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NOTES AND COMMENTS.

Science—Educational and Professional.

AT the recent opening of the new buildings for the Department of Human Anatomy at Oxford, a question of considerable interest came into prominence. As everyone knows, Science has two sides, and the two sides, except in the most elementary stages, have a very different aspect so far as study is concerned. All the natural sciences have their applied side. Geology is concerned with agriculture and with mining; Botany with agriculture, with pharmacy, forestry, and so forth; Anatomy and Physiology with medicine; Zoology with veterinary work, pisciculture, etc. As purely educational subjects and as ends in themselves leading to research and the advancement of knowledge, they are at least equally important. The one side appeals to the practical man—the follower in philosophy of Bacon; the other to the idealist, the follower of Plato. But it is a hard world, and as the practical side offers a livelihood, students are attracted to it. This rules especially in the department of Biology, as a medical career attracts so many men. What is a University to do? Its highest function certainly is the Platonic side of science, and the great endowments of the past would seem best used when directed to learning. But what are professors and laboratories without students? Cambridge effects a reconciliation by having a large medical school and by giving many fellowships to Biology men, who research and batten on the fees of medical students. Oxford has a small but growing medical school, but the few biological fellows she has had, have with all haste shaken the dust of Oxford from their feet.

Demonstrators at Oxford.

Another pressing problem at Oxford is the status of demonstrators. The habit of the University has been to consider the demonstrators as mere personal appanages of the Professors. The result of

that habit has been that almost every change in the professoriate has been followed by an alteration in the staff. As a general rule, that alteration has been such as to substitute for Oxford graduates, graduates of other universities. Even apart from this, very few demonstrators have stayed at their posts longer than a year or two. So far there are at once advantages and disadvantages in this custom. But what is an unmixed evil, and what is being agitated against at present, is the absolute want of University status possessed by demonstrators. Appointed and removed at the will of the professor, unrepresented on the Councils of the teaching staff, practically unrecognised by the University and totally unrecognised by the Colleges, the posts have nothing to compensate the meagreness of the stipends. If the leaders of Oxford University really want to stimulate Science at Oxford there is a full programme of reform before them. The planks in such a reform might well be:—

Increase of Scientific Fellowships.

Increase of Stipends of Demonstrators.

Improvement of Status of Demonstrators by

(1.) Recognition by the University.

(2.) Recognition by the Colleges.

(3.) Admission of Demonstrators, at least when these are Oxford graduates, to the Board of Faculties, and to the Museum delegacy.

In a word, demonstratorships should be University appointments: like other University appointments, they should be attached to colleges, so that by virtue of office a demonstrator should become a member of some senior common-room.

Science as She is Taught.

Being anxious to obtain some information about popular encyclopædias, we recently set our office-boy to study and report upon them. Our first selection—Saxon's "Everybody's Scrap-Book of Curious Facts," by Don Lemon—was apparently not a fortunate one, and the following extracts will show that our office-boy's report that the statements therein contained are more correctly described as "curious" than as "facts," was not without justification. We told him to commence with the Natural Sciences, and he was naturally startled by finding our respected contemporary, the *Zoologist*, made responsible for the statement that whalebone forms the substitute for teeth, and that sheep have no teeth in the upper jaw; we shall expect a libel action to result from this. The author, however, accepts himself the responsibility for making centipedes into reptiles (p. 189), and placing the teeth of the sea-urchin round the stomach (p. 300), and for revealing to geologists (p. 47) that granite "is from two to ten times as thick as the united thicknesses of all the other rocks," and that from granite all other rocks have been derived; if he stopped

here we might have been grateful for the information, but as he goes on to talk about *atoms* of lime, and that granite contains none, our faith is shaken. Of course, many fine old-crusted "Münchausenisms" are trotted out anew, such as the 5,000 year old baobabs (p. 173), and the raining of blood (p. 259); confirmation of the existence of the giants on the earth in those days is still derived from the bones of the Irish Elk (p. 260), odour is yet regarded as immaterial (p. 8), and the author seems to have no suspicion that the humming of insects is due to any other cause than the flapping of the wings. Perhaps it is hardly worth calling attention to so slight a mistake as not knowing the difference between Newfoundland and Labrador, which must have given rise to the statement on p. 128 that the interior of the former is still a *terra incognita*. Geography is not the author's strong point; but neither is mathematics, or he would not have left (pp. 302, 303), as an inscrutable problem, the "curious arithmetical puzzle" of the herring-and-a-half for three halfpence order. We are, therefore, not surprised when he tells us the atmosphere is saturated when it contains 84 per cent. out of a possible 80 per cent. of moisture; in these conditions we should think it as supersaturated as the compiler's brains. Nor ought one to grumble that, when he tries his hands on crystallography, he fails to distinguish between lapidaries and crystallographers, and in describing garnets mistakes polygons for polyhedra (pp. 131, 132).

His "facts" are not only at times inconsistent with the facts, but they are often inconsistent with one another. Thus on page 212 he tells us first that Sunken Lake is the deepest in the world, and next that Lake Baikal is "by far the deepest lake in the world." If he had only put these facts a few pages apart, the office boy would doubtless have done his best to believe them; the juxtaposition of contradictions in this case is the more surprising, as elsewhere the illustrations of the truth of a remark are often given in quite a different part of the encyclopædia: thus he heads one paragraph "statistics are funny" on p. 55, while his calculation as to the population of ancient Rome which proves it, is delayed till p. 80. In spite of all the vast range of the author's knowledge he is very modest and loves simplicity: thus his formulæ on p. 44 for determining ships' tonnage ought to charm the heart of Lloyd's with their exquisite ease and wide applicability; while his confession on p. 19 that he will never be able to understand the principle of the steam ejector shows that he does not rate his own powers very highly; but when he sums up "what's a flame?" in four lines beginning with "Combustion is in some way produced by the union of carbon and hydrogen with oxygen," and ends with "the best philosopher can tell little more," we cannot but excuse our office-boy's comment of "More things in heaven and earth can be explained than by your philosophy, Don Lemon." The author's use of the word "fact" is certainly strangely comprehensive, for we do not quite see how advice as to the selection of a wife (however sound in itself) can be

regarded as such; but we cannot grumble at this as, if it were excluded, so might have been the remarks of Professor Blackie, which ought, in future editions, to be rescued from the obscurity of p. 51, and printed in large type on the title page:—" Never force yourself to learn what you have no talent for. . . Be content to be ignorant of many things, that you may know one thing well." This collection of curiosities has, however, we understand, an enormous sale, and its statements are doubtless accepted as "facts," whereby we are reminded of Carlyle's estimate of the population of the British Islands, " 30,000,000 mostly fools!"

A Recent Submergence of Western Europe.

Professor Prestwich has communicated to the Royal Society a paper " On the Evidences of a Submergence of Western Europe and of the Mediterranean Coasts at the close of the Glacial or so-called post-Glacial Period, and immediately preceding the Neolithic or Recent Period." (*Proceedings*, vol. liii., pp. 80–89.)

He points out that certain superficial deposits, containing organic remains of a land-surface, and composed always of local materials, are found to radiate from independent centres and to show no signs of glacial action. They are grouped by the author under the general name of " Rubble Drift." This assumes various forms.

As "*Head*" it overlies raised beaches by the Channel, in France, Jersey and Guernsey, and on the North African Coast eastward to Tunis.

As *Osseous Breccia* it covers the lower slopes of the rock of Gibraltar, extends round the borders of the hills encircling Palermo's plain, and is found beneath the high escarped rocks of Malta and in Greece.

In *Ossiferous Fissures* it occurs on isolated hills along the Riviera, in Southern France, at Gibraltar and in Algeria.

As *Loess*, apart from that which occurs within valleys and is due to river floods, it is found on dividing watersheds and the high plains of West and Central Europe.

Differing though this Rubble Drift does in detail, the author considers it to be due to a common cause, and only explicable upon the hypothesis of a submergence, which was at once widespread and short in duration. It allowed no time for marine sedimentation, while the establishment of a marine fauna was prevented by the turbid character of its waters. The resulting deposits come from the wreck of the land-surface only. This submergence was followed by an upheaval, since which the configuration of the land has remained comparatively unaltered.

The ossiferous fissures, found often on isolated hills, in France, at Gibraltar, and elsewhere, contain the remains of carnivores, ungulates, and ruminants. After showing that these remains could not have been collected by beasts of prey, the author points out that

only a great and a common danger could have driven together such variously-natured animals. Such danger would be found in advancing waters, driving the animals to take refuge on isolated hills, or in other circumscribed areas, there to meet their deaths by drowning. It is noticed that while certain animals did not survive the rubble drift in the west, they existed to historic times eastwards in Egypt—of the submergence of which country there is as yet no evidence.

When the upheaval followed, portions of the surface *débris* were swept down, detached bones were carried away, other bones were crushed and splintered by rolling fragments of rock; and the drift deposits were dispersed from various centres by the diverging currents that resulted from the gradual elevation of the sea-bottom.

Nuclear Division in Branchipus and Apus.

Mr. J. E. S. Moore, in the *Quarterly Journal of Microscopical Science* for September (vol. 35, pp. 259–283), has published some results of great interest. In the male gland of Branchipus, the first stage in spermatogenesis consists of the enlargement of the cells lining the tube, and the arrangement of their nuclei into a spirem with the chromatic elements all on one side. During this time, a large number of bodies like centrosomes (pseudosomes), appears in the protoplasm at the cell-basis. The spirem breaks up into ten dumb-bell shaped chromosomes, and these arrange themselves in an equatorial plate. But in this process the chief interest is that the observer regards it in the light of Bütschli's froth theory of protoplasm. At first the whole nucleus appears to be made up of drops of a clear fluid, enclosed in films of staining material. These drops gradually run into each other, as the drops of a soap foam grow larger by smaller bubbles breaking into the larger. As this process goes on the strands or meshes of staining material become thicker and thicker, and ultimately break up into the chromosomes. As this running together of the nuclear bubbles progresses it breaks beyond the nuclear limits, and bursts into the cytoplasm. The chromosomes thus are left hanging in a clear plasm, supported by a few strands of the cytoplasm which have not yet been broken through. These irregular strands become reduced to fine threads, and the threads terminate in the pseudosomes. As a result of the tractions of the films of the bursting bubbles, the threads with the chromosomes become pulled into a spindle of which the poles are those pseudosomes which have "the best foothold," on the cell periphery. The other pseudosomes and their threads converge on these, and the result is a more or less regular spindle. The actual centrosomes are a fusion of some of these pseudosomes, and are thus in origin equivalent to the angular spaces in the network exterior to the nucleus. A progressive fusion of the bubbles in the cytoplasm sweeps together the scanty stainable material of the cytoplasm into

twenty bodies called *dictyosomes*, just as the fusion of the bubbles in the nucleus swept together the nuclear chromatics into the chromosomes. Thus pseudosomes, centrosomes, and dictyosomes, as well as chromosomes themselves, are all expressions of the froth-like activity of protoplasm.

It is of much interest to find an observer of actual miscroscopic details in karyokinesis connecting the actual observed processes with, on the one hand, Professor Bütschli's Foam Theory of protoplasm, and, on the other, Professor Weismann's idants and reducing divisions. It is truly bringing the phenomena of heredity into close relation with physical phenomena.

The Origin of Dicotyledons.

Botanists as well as geologists will welcome Professor Lesquereux's posthumous monograph on the Flora of the Dakota Group (*Monographs of the United States Geological Survey*, vol. xvii.). Few facts are so puzzling to the evolutionist as the sudden appearance in profusion in Middle Cretaceous times of highly-organised dicotyledons, often belonging to existing genera. The older strata, even the Wealden, give no hint of the existence of any plants of such high type; and yet the Cenomanian deposits of Dakota have already yielded no fewer than 429 species of dicotyledons, without including any herbaceous species, for little has been preserved except leaves of deciduous trees. When the discovery in Dakota of these remarkable deposits was first announced, there was a natural hesitation to accept their Cretaceous age; but, since the stratigraphical evidence has been made clear, botanists have been awaiting the appearance of figures and descriptions, to show to what extent the reference of so many of the plants to still living genera is justifiable. Now that this handsome monograph has been issued, we are better able to judge, and one cannot help being struck with the close resemblance of many of the leaves to those of existing plants; yet, on the other hand, one observes that nearly all of the species are described from leaves alone, and that the few detached fruit yet found have nearly all to be left in the indefinite genus *Carpites*, for they do not seem to be closely allied to recent genera. According to Professor Lesquereux's analysis, the flora of the Dakota group is composed of 460 species, of which 6 are ferns, 12 cycads, 15 conifers, 8 monocotyledons, and 429 dicotyledons. Such a proportion of dicotyledons is far more suggestive of a recent flora than of one of Cretaceous age, and it is singular how few characteristic Secondary genera have been obtained. Professor Lesquereux's suggestion, that we are enabled to refer the origin of the dicotyledonous plants to the beginning of the Cretaceous period, no doubt accords with the facts, as far as they are yet known; but both botanists and geologists will hesitate to adopt it, in face of the highly differentiated dicotyledons existing in the Dakota group.

A Remarkable Orchid.

Mr. F. W. Moore, the enthusiastic Curator of the Glasnevin Gardens, Dublin, has, according to the *Orchid Review*, recently flowered, for the first time in Europe, a plant of *Coryanthes Wolfii*. This remarkable genus attracted the attention of Charles Darwin, who gives a figure of the flower, and describes its strange method of fertilisation, in his work on the Fertilisation of Orchids. It will be remembered that drops of liquid are excreted and caught in a bucket-shaped development of the lip, that bees struggling for a place on the edges of the bucket get pushed in, and their wings being wetted by the liquid, have no means of escape except by a narrow passage which is arched over and temporarily closed by the anther and stigmatic surface. The present species comes from Ecuador, and was named by Mr. Lehmann, the German Consul in the United States of Colombia, well-known as an energetic botanist and orchidologist. He says, writing in the *Gardeners' Chronicle*—"It grows very sparingly, mostly on cacao trees, all over the littoral districts of the Guayas, where it flowers in February and March, when these level lands are mostly inundated. During this season it is beyond the power of man to penetrate the woods there—a circumstance that accounts for the plant not having been seen before. It produces thick upright flower-spikes, 40 to 50 cm. high, with three to six large, wonderfully-constructed flowers, which are yellow, mottled, and stained with brownish red. There are but few plants in the entire vegetable kingdom which are more interesting, and which afford such a varied amount of material for the student of vegetable physiology." The plant has a strong attraction for ants, numbers of which surround its root masses, and are apparently essential to its well-being, as in their absence it seems to do badly. This myrmecophily was observed not only in the tropical South American forests, but under cultivation. The ant is a small species of *Myrmica*, with a strong aromatic smell, and a bite so severe that it requires some courage to meddle with the plant.

Names of Orchids.

In the same journal, which, by the way, contains much to interest the orchid-lover, are descriptions by Mr. Rolfe of new species of *Masdevallia*, *Laelia*, and *Maxillaria* which have flowered under cultivation, and also a note of warning to those who are apt to trifle with nomenclature. *Cypripedium spectabile* is one of the best known and most beautiful of Cypripediums, but it has no right to this name, under which Salisbury described it in 1791, as in so doing he ignored two earlier names, *C. album* of Aiton (1789), which seems to have been discarded as inapplicable because the lip is rose-coloured, and *C. reginæ* under which it was described by Walter in his "Flora Caroliniana" in 1788. *Cypripedium reginæ*, as we must therefore call it, and none will deny the suitability of the name, has a remarkable

distribution. Hitherto known only as a native of the peat bogs of Canada and Eastern North America, it has recently been discovered in Western China, on the borders of Tibet, together with the remarkable little *C. arietinum*, also a native of the same parts of North America.

As a genus, *Cypripedium* has fared somewhat badly with the people who discuss nomenclature. In his review of Mr. Jackson's monumental "Index," the editor of the *Journal of Botany* refers to the omission of Ascherson's emendation of the Linnean name, to wit *Cypripedilum*, launched in the Flora of Brandenburg in 1864 to accord with the etymology (*πέδιλον*, a slipper), since *Cypripedium*, as the German doctor observes, "ist nicht zu erklaren." It is to be regretted that Pfitzer should have taken up Ascherson's name in Engler and Prantl's "Pflanzenfamilien" and elsewhere.

The practice of amending names the etymology of which is not clear to the emendator's mind is a reprehensible one, and apt to lead to still greater confusions. Thus, to correspond with Ascherson's alteration, Pfitzer writes *Selenipedilum* for *Selenipedum* of Reichenbach *fil.*, and also establishes a third genus—*Paphiopedilum*. The new genus must stand, but the other two return to their original form, and so the symmetry is destroyed. Another instance of the same intermeddling is supplied by Lestiboudois' *Heleocharis* for Robert Brown's *Eleocharis*, to explain its derivation from ἕλος a marsh, and dispel any idea of pity (ἔλεος) or a kitchen table (ἐλεός).

American Botanists in Congress.

The September number of the *Botanical Gazette* is filled with reports of meetings. The American Association for the Advancement of Science met at Madison, Wis., on Thursday, August 17, and sat till the following Tuesday. Professor C. E. Bessey presided over the botanical section, and the *Gazette* prints an abstract of his address, as well as an outline of the papers and discussions. In his address, entitled "Evolution and Classification," the Professor upbraids systematists for their unscientific conservatism in retaining the crude system of Jussieu and De Candolle for the arrangement of flowering plants for more than thirty years after the general acceptance of the doctrine of evolution. Evolution has taught us what relationship means, and from the new point of view a natural classification is not merely an orderly arrangement of similar organisms, but an expression of genetic relationship, in which primitive forms will precede those derived, and the relationship of the latter be positively indicated. Just as Carl Linné's artificial system was in general use among botanists long after the construction of a natural system by Jussieu, so in turn this natural system has persisted by the help of conservatism and reverence, till ceasing to be natural it has become a makeshift, and "is now as much a clog and hindrance to the systematic botany of the higher plants, as was that of Linné sixty years ago."

There is doubtless a great deal of truth in these remarks, but what has Professor Bessey to offer us in place of our present system? It was admitted from the first that the Apetalæ formed an artificial group; a residue, in fact, of families whose places in a natural system could only be determined by further study. This study is now progressing. Treub has shown, by his work on *Casuarina*, that one family at any rate has affinities with groups other than the Polypetalæ among which Professor Bessey would apparently distribute all the apetalous orders. Quite recent work on the Cupuliferæ has led to somewhat similar results, and we cannot but think that until other families of doubtful affinity have been subjected to the same close investigation, it will, on the whole, be better to keep them apart. Of course in the case of those forming the series of Curvembryeæ, and others, relationships are more apparent, and there is no objection to ranking them with their polypetalous allies; but until a more truly natural system has been satisfactorily elaborated, systematists will not be willing to drag to pieces their herbaria, repeating the process every six months to suit the peregrinations of a few orders of doubtful affinity.

FOLLOWING the botanical proceedings of the Association is an account of the proceedings of the Madison Meeting of the Botanical Club connected with the Association, in which the check list ordered to be prepared by the committee on nomenclature occupied the chief place in the discussions. The Botanical Club met on four occasions during the session of the Association. Finally, we have the proceedings of the Madison Botanical Congress, which occupied two days immediately after the close of the two former functions. The desired international character of the assembly was not realised, the attendance of European botanists falling much below the expectations of the organising committee, and it was therefore decided that questions of nomenclature should not be discussed. Professor Greene was elected President, and M. Henry de Vilmorin, of Paris, one of the Vice-Presidents.

IN the September number of the *Journal of Botany*, Mr. Britten makes some amends for the omission of Gilbert White's name from the "Index of British and Irish Botanists," by conclusively demonstrating his claims to be included. A copy of Hudson's "Flora Anglica" has recently come to light with White's autograph on the fly-leaf, and a note, also in his hand, that all plants occurring within the parish of Selborne are marked by a cross. Several corrections and a few MS. notes show that he used the book a great deal. Moreover, Mr. Bell, in his edition of Selborne, states that he had a catalogue of Selborne plants in White's handwriting. At the end of his note Mr. Britten prints a list of the 439 species indicated by the Selborne naturalist.

In our last number we briefly drew attention to the stirring address by Mr. Teall on Uniformitarianism: an antidote has already been provided.

In an article on "The Position of Geology" (*Nineteenth Century*, October), Professor Prestwich offers a vigorous protest against "the dwarfing influence of Uniformitarianism." He admits that the forces acting upon the surface of the globe in past times have remained the same *in kind*; but he strongly opposes the notion that they have remained the same *in degree*. "What (he asks) if it were suggested that the brick-built Pyramid of Hawâra had been laid brick by brick by a single workman? Given time, this would not be beyond the bounds of possibility. But Nature, like the Pharaohs, had greater forces at her command to do the work better and more expeditiously than is admitted by Uniformitarians." Professor Prestwich complains, and we think rightly, that many arguments with regard to geological time and the rate of denudation are based on very limited evidence; they furnish standards applicable to present changes, but they give no measure of the amount and rate of work that could be done. We must interpret phenomena by the light of the facts themselves. Those who claim vast periods of time for the Glacial and post-Glacial periods, give no explanation why such animals as the Reindeer, the Musk-ox, and the Glutton survive unchanged. He concludes that Uniformitarian measures of time "have probably done more to impede the exercise of free inquiry and discussion than any of the catastrophic theories which formerly prevailed"; they "hedge us in by dogmas which forbid any interpretation of the phenomena other than that of fixed rules which are more worthy of the sixteenth than of the nineteenth century."

"The Work of the Geological Survey" forms the title of a paper read, by Sir Archibald Geikie, before the Federated Institution of Mining Engineers (*Transactions*, vol. v., p. 142). After a short sketch of some early geological maps, the writer gives an account of the method of mapping, and of the various kinds of map issued by the Geological Survey. Other branches of the Survey work are dealt with, and it is stated in conclusion that "From the beginning of its existence the Survey has been continually referred to by all branches of the Government Service for information regarding questions in which a knowledge of geology is required. The sinking of wells, the choice of sites for forts and Government buildings, the placing of graveyards, the selection of materials for buildings or roads, the nature of soils and subsoils, with reference to matters of drainage—these and many other subjects have been reported on. Nor has the general public been backward in application for similar information. The offices of the Survey are always open, and every assistance which can be rendered to enquirers is placed freely at their service."

An important memoir on the Jurassic rocks of the Southern Jura by Dr. Attale Riche has lately been published (*Annales de l'Université de Lyon*, vol. vi., 1893). The beds described are the Bajocian, Bathonian, and Callovian, which the author includes as "Jurassique Inférieur." In this respect he departs from the ordinary grouping, which places the Lias in the Lower Jurassic. The local details furnished by the author are valuable; while his Comparative Table of formations, which places beds with *Ammonites parkinsoni* on a level with those yielding *A. arbustigerus*, is calculated to yield matter for discussion. In this way *A. parkinsoni* comes in both Bajocian and Bathonian formations. He ends the Lias with the zone of *A. opalinus*, and includes the representatives of our Cornbrash in the Callovian. The work shows the difficulties that attend all attempts at very minute correlation of strata in different areas.

A short paper just published by Mr. W. H. Dall is likely to be overlooked as of purely palæontological interest unless his last paragraph is read. Mr. Dall, writing on a subtropical Miocene fauna in arctic Siberia (*Proceedings of the United States National Museum*, vol. xvi.), remarks that "it is perhaps very late in the day to refer to the hypothesis which explained the warm water Old Miocene of the North Atlantic shores by assuming a shifting of the polar axis, so that the pole at that time would have been situated somewhere in central Siberia. That hypothesis has few, if any, friends at the present time; but it may not be amiss to point out that, if it were necessary to put a quietus on that moribund speculation, the presence of a warm water Old Miocene in eastern Siberia, such as our present fossils indicate, would be quite sufficient to prove that no polar conditions in the modern sense could have existed there during that epoch of geological time."

Subtropical, or at least warm-temperate, Miocene plants have already been obtained in Greenland and Spitzbergen, and the North Pole was evidently completely encircled by a warm belt such as no shifting of the axis will account for.

Authors should, if possible, give a clue in the titles of their papers to the subjects dealt with. In the *Geological Magazine* for October, we find the first part of a paper on "Some Cretaceous Pycnodont Fishes," by Mr. A. Smith Woodward. Imagine our surprise when we discovered in it accounts of Kimmeridgian, Portlandian, and Purbeckian Fishes! Some Neocomian and Cretaceous forms are also described.

I.

Geology in Secondary Education.[1]

THE need for a selection of subjects in modern education becomes pressingly apparent, and there is consequently every danger of specialisation at too early an age. The result of this will be that men and women will grow up as students of some branch of natural science, literature, or mathematics, with even less in common than is the case between the several groups of educated persons at the present day. While every thorough worker, knowing the influence of his own favourite study upon himself, is apt to put forward the claims of that study as the real essential in educational progress, it is with all seriousness that I assert that, in gėneral secondary education, Geology should receive a recognised position.

Elementary Chemistry and Physics may be accepted as being now taught in all self-respecting institutions, such as the great day-schools, the endowed boarding-schools—which often usurp the title of "public schools"—and similar progressive seats of learning. In the following suggestions, everything that is put forward is intended to apply equally to girls as well as to boys, the time having long passed when it could be maintained that knowledge which enlarged the views of the one sex would unfit the other for the affairs of life. This paper is also aimed somewhat at the future, when secondary education will be brought within the reach of all classes, perhaps by a wider system of scholarships open to pupils of the board-schools.

Given a fair grounding in the facts of elementary chemistry and physics, the application of these facts to the great natural world on which we depend leads us directly to geology. The common minerals, the common rocks, their common modes of association, are capable of direct observation, and offer material for consideration wherever the future lot of the student may be cast. I am now taking the case of the ordinary individual, whose leisure-time is becoming in general increased. The correct understanding of one's surroundings is capable of adding a new pleasure to existence, and of providing a rational occupation—which is one of the great ends of education.

The amount of knowledge that the average schoolboy possesses about the Punic Wars, or the externals of Greek mythology, is some-

[1] An introduction to a discussion at the meeting of the British Association at Nottingham, September, 1893.

thing awe-inspiring, at the least; yet he comes up to a University in ignorance of what has made the features of the landscape which he has seen all his life from the door of his country home, and of what agents have brought together the pebbles of his summer bathing place.

If the utility of such knowledge be questioned, we open up an attack upon the vast body of information crammed into unsuspecting childhood. The only answer is to admit the utility of all knowledge whatsoever, and then to assert the special desirability of that branch under consideration.

At the present moment much attention is given to technical education, which has two main objects—the improvement of the workman and the improvement of the object produced; but there are many who look to such education primarily as a means of acquiring wealth—and also of competing with the Germans. In this paper I regard education as a means of acquiring happiness and of living peaceably with one's neighbours; so that the financial return may be disregarded until special cases are considered in the discussion. National secondary education, I take it, should insist on the dependence of the individual on his fellows rather than on the dependence of the masses on an individual; in other words, it should enable a man to realise his position as a member of a community, to live without grasping, and happily upon a moderate income.

Now Geology becomes indispensable for a correct appreciation of our relations to our surroundings and to the past. In a general course of instruction during, say, the last year of secondary education, when the pupils are of the age of 16 or 17, I would treat of the different minerals of which rocks are made, always bearing in mind the materials to be found in the locality in which the teaching is being given. I would deal as little as possible with the more refined aspects of these minerals—such as their crystalline systems—beyond an indication of the facts of symmetry and of how minerals, apparently similar, may be ultimately distinguished by their crystalline forms. I would lay much stress on the character of hardness, using the thumb-nail and the pocket-knife, and on simple chemical means of determination. It may be worth noting that I have always found girls most apt in matters requiring the elements of crystallography, while boys, as a rule, are indifferent to symmetrical beauty.

In dealing with rocks, I should exclude the use of microscopic sections. For our broad untechnical purposes, it would seem better to grind down a surface when structure has to be exhibited, and to pick a rock to pieces, with the aid of a hand-lens or a dissecting microscope, when its constituents have to be determined. The methods used in more elaborate work might, however, be hinted at; but in every week's lesson the teacher will have to remember that he is training men and women, and not miners or geologists.

The mineral analysis of some ten common rocks may thus be

discussed; the teacher will do well to avoid debatable terms, or those dependent on purely microscopic examination. Every encouragement should be given to the idea that rock-specimens are parts of the great masses round us, and that Geology is a study of the open air, natural in the highest sense, and not an exercise in more or less classical terminology.

The modes of origin of these rocks may be described side by side with their mineral characters, an amount of physical geology being thus introduced to enliven the bald statement of their constitution. Thus an account of modern marine shell-banks may precede the discussion of limestone, or may proceed naturally from the observation of fossils in a rock; while an account of the main features of an active volcano will add vast interest, and even dignity, to the details of a lump of basalt.

The structures of mountains, the carving of valleys in high plateaux, the rounding of pebbles, and the sifting of materials in the streams, can all be illustrated by local examples, or by reference to the customary excursion-resorts of the people. London and the south-east of England form a district less favoured than most parts in the matter of variety of structure; yet the broad facts of physical geology can even there be dealt with practically.

But when the proofs of what were formerly called "revolutions" in the surface of the globe have been made plain, and the slow and gradual nature of those changes has been impressed upon the student in the field, the review of the past history of life becomes the main object of his study.

For the public at large, this portion of the subject has, to my thinking, the greatest educational value; I urge, indeed, the fundamental importance of geology on account of the desirability of giving everyone, from the peer to the proletariat, an outline of the history of life upon the globe.

It is impossible to disguise the fact that the teaching of human history is liable to be both partial and partisan; and that, under existing systems of examination, a pupil may be well read in the events of one century of the history of his own people, and yet be entirely ignorant of the causes that have moulded the popular opinions of his own time. Firstly, then, Geology introduces the student to a history in which one is not called on to take sides, a history the compilation of which is the outcome of enquiry rather than enthusiasm—a remark that holds good, at any rate, until we reach the Glacial Period. The pupil learns, then, the beauty of facts, and that what we call truth is a conscientious and necessarily imperfect deduction from them. The very imperfection of the record will cause him to reason for himself, and to recognise fully the difficulties of the process. Thus, perhaps, his judgment of things in general will become less personal, less harsh, and less redolent of the dogmatism of a "school."

But any natural science will similarly benefit and broaden our egotistical souls; the peculiar claim of Geology is its direct bearing upon history. To the average schoolboy the Greeks and Romans divide the honours of antiquity; the classical models of heroic virtue are so marked out and emphasised that it is often doubted, in serious works and popular lectures, whether humanity has altered for the better since the days of Horatius Cocles. The gloomy remark that human nature will always be human nature is continually regarded as an argument in favour of repressing the species, rather than of providing loopholes for its development. To present the ordinary man or woman with a picture of the Palæolithic epoch, and at the same time with a history of life in which, on any reasonable scale, the whole of recorded human history, including the Chinese, is too short to be taken as a unit—this surely is calculated to give one room for hope, and to shake one's faith in the immutability of human nature.

One more point of view. The enormous past, which we cannot deny to have been progressive, tends to give a certain solidarity to our conception of the human race. Man becomes a prominent feature along a particular and limited horizon of the earth's history, just as the Romans did in the recent history of Europe. The race acquires an importance to the geologist, standing, as it does, at the end of so vast a series of life-changes; and he who learns to respect the race may in time become less callous about the extinction of an individual. I do not mean to assert that a diffused knowledge of, let us say, the Eocene Period would have prevented the hecatombs of Waterloo or Weissenburg; but it would have gone far to make such tragedies more generally regretted, and more generally hateful. The stratigraphical position of man, when it is once realised, makes him appreciate both his present possibilities and his dangers. If Geology helps him to recognise the differences between groups of men, as, at the most, specific rather than generic, the study will have done much to promote the harmony of nations.

I would ask, then, those interested in Geology to watch the progress of public education, and to endeavour to secure a place for this science side by side with the historic and human studies of later years of the curriculum. Personally, I have found it most interesting to note the intrusion of completely novel ideas—in my own case in Colleges for women—when such a subject is superposed on the ordinary courses for an Arts degree. Although the foundation of Chemistry and Physics is still in many cases slight or absent, the results, as far as I can observe them, are full of encouragement and hope.

GRENVILLE A. J. COLE.

II.

Natural Science at the Chicago Exhibition.

SCIENCE of any kind is lamentably absent at the World's Fair. In this century, which we delight to patronise as "The Age of Science," and in a country where the educational method of exhibition has been brought to such perfection as it has in the larger American Museums, one would have expected something more than a heterogeneous assemblage of exhibits, some good, many bad, and most indifferent, which, in the expressive language of the day, are planked down anywhere. Organisation and arrangement must have expended all their strength on the outward appearance of the Fair, which is indeed admirable, and have had none left to tackle the far more important problem of displaying the exhibits in the most instructive manner. In this respect the only satisfactory building is that belonging to the Government of the United States; and had the same intelligence which directed that, been, as was originally intended, brought to bear on the rest of the Exposition, the result would have been as gratifying to the earnest student and the scientific visitor as it now is to the pleasure-loving and wonder-seeking public of the World.

Our concern, however, is more particularly with those branches of Science known as Natural, and it may prove instructive to consider how such are represented at Chicago. The exhibits of this kind may be divided into the Natural Science exhibits and the Natural History or semi-scientific exhibits: the latter largely preponderate. As no one visitor can hope to discover everything of interest to him that may be hidden in the nooks and corners of the White City, we have not scrupled to draw when necessary on an excellent article that appeared above the well-known initials of W. H. D., in *The Nation* for September 14, 1893; quotations therefrom are distinguished by inverted commas.

"In ZOOLOGY the Fair offers a rather meagre display, which is made still more obscure by being broken up into small exhibits, many of which are found in most unexpected places." Most of them, however, are, or are supposed to be, contained in the Anthropological Building, and among these the most representative general exhibit is that of Professor Ward of Rochester. "Here a very excellent series of specimens suitable for a teaching museum are brought together, well and clearly labelled, and attractively arranged. The model of the

mammoth here attracts much attention, and the taxidermy throughout is good, if not remarkable." Among the Invertebrata of Professor Ward's collection, the Corals and Echinoderms, notably the dried and beautifully-mounted specimens of *Pentacrinus*, are worthy of attention: for exhibition purposes these latter are certainly superior to spirit-specimens, as they can be clearly seen. In the East Gallery of this building are the faunal exhibits of Maine, Ohio, Colorado, and Ontario; also "a number of specimens of taxidermy exhibited by private persons, none of which needs special comment; the general average is poor and inartistic. The furriers in the Liberal Arts Buildings have here and there a well-mounted fur animal; a few may be found in the Leather Building; and small collections representing State faunas may be found in most of the buildings erected by the several States or foreign countries. Most of these are poorly mounted and very imperfectly labelled. Occasionally a rare creature may be detected among these forlorn representatives of their kind, as in the case of the Liberian hippopotamus, a mounted specimen of which is included in the exhibit of that little African State. The exhibit made by the State Museum of New York is creditable, clean and well labelled, one of the best of the minor collections." The collection of stuffed animals shown by British Guiana is also worthy of special mention.

It is, as above indicated, in the Government Museum that the most scientific exhibits are to be looked for. Here "the National Museum exhibits a mounted series showing examples of all the families and most of the genera of American mammals and a number of groups of North American mammals, accompanied by accessories indicating the natural surroundings of the species. Among these the most interesting are those containing the Rocky Mountain sheep and goats, the woodland and barren-ground caribou, and the Pacific sea-lions. A fine walrus might have been more life-like if the taxidermist had had a better guide than Elliott's caricatures of this unfortunate animal, which, in addition to extinction, seems to be doomed to posthumous misrepresentation. The most important among the exhibits in this series is the mounted skin of a very good example of the Alaskan sea-otter, perhaps the best existing specimen in any museum. A number of large African game animals are also shown, some of which are rare. Similar series, illustrating the American families of birds, reptiles, and batrachians, fishes, insects, etc., are also exhibited by the Museum, as well as two very attractive collections of the birds of Paradise and humming-birds. Several groups of species, formerly common but apparently now verging on extinction, comprise the Carolina paroquet, the wild pigeon, and the ivory-billed woodpecker. There are also spirited groups representing the courtship of the prairie chickens, and the flamingo with its singular nests." We, in England, are familiar with such natural groups of birds, and excellent examples of this sort of

mounting may be seen at the American Museum of Natural History, New York; "but the restricted space and the small amount of money available at Washington have hitherto prevented the National Museum from extending to birds the treatment which has been so successfully applied to the larger mammals." The popularity of this Smithsonian exhibit may be gauged by the difficulty that a visitor experiences in forcing his way through the almost immovable crowd. One portion, however, he will have almost to himself; yet it is by no means the least interesting. This is the admirable Synoptic Series of Invertebrata, arranged by Mr. Lucas, where every help is afforded by means of well-expressed labels. Adjoining is the no less instructive series illustrating the osteology of the vertebrata. This shows the homologies of the principal bones by means of compared articulated and disarticulated skeletons of the chief vertebrate types, the bones of the cranium being distinguished by colours.

Those who visited either the Fisheries Exhibition at South Kensington, or the similar division of the Philadelphia Exposition, will learn little new at Chicago. The Fisheries Building is truly, as it has been called, "a poem in architecture"; but, except for the fisheries of N. America, the exhibition is small and mostly unimportant. "The reason," writes Mr. Dall, "is not far to seek. The United States and Canada collectively have nothing to learn at present from foreign countries, and no foreign dealers in fish products have any reason for supposing that they can gain a foothold in our markets, except for sundry specialties like cod-liver oil. Consequently the commercial incentive is lacking, and, apart from such countries as New South Wales and Japan, which have made an exhibit as a matter of national pride and in evidence of the state of their industries, the foreign fisheries are very imperfectly represented." The exhibit of the Fish Commission in the Government Building is good, "though less prominent than in 1876, and the practical rather than the scientific side of its work is emphasised. Similarly, the collection shown by Japan is more interesting from the economic or anthropological side than as illustrating the ichthyology of the empire." Pounded shark and dried cuttle-fish do not strike the Western mind as particularly appetising; but with cockroach catsup they are by no means bad. One doubts, however, whether the enterprising Japanese firm who are trying to introduce dried sea-weed as a relish will meet with the success that they deserve. Some of the Japanese methods of fishing are so curious that they might have been illustrated at Chicago, but, as they are not, one cannot now describe them. "There are a few interesting models of fish traps and weirs in the collection shown by the Sultan of Johore in the Plaisance, but these objects belong under the head of Anthropology rather than that of Ichthyology."

Among the more obviously attractive exhibits in the Fisheries Building are the skeleton of a Pacific Humpback Whale, 47 ft. 6 in.

long, and 48 ft. in girth, the tent used by Lieut. Peary on his present Arctic expedition, and the Aquaria. In these latter the foreigner will probably be most attracted by the fresh-water fish of America; while, as Mr. Dall remarks, the visitor from the Western States will learn much about the salt-water fish so unfamiliar to him. Certainly, it was round the sea tanks that crowds congregated. The fresh-water tanks have, unfortunately, suffered from virulent attacks of *Saprolegnia*, the fish fungus. "This exhibit, as a whole, is that of tanks of water with fish in them, and not aquaria in the strict sense of the word, which implies a balance of conditions between the water plants and the animals, as in a state of nature."

"Many of the States show more or less dilapidated stuffed fishes as a part of their faunal exhibit, but California has an excellent series of models, by Denton's method, of her chief economic species. These are well coloured and properly labelled. New South Wales has a good series of alcoholic fishes, in the same excellent order as the rest of her exhibits," as well as a striking set of coloured drawings.

"The shell-fisheries are almost unrepresented in the Exposition except by the canned product. A few unlabelled and poorly-preserved shells appear in the Brazilian exhibit, a few oysters and pearl shells in the collections of New South Wales and Mexico; Wisconsin shows a pretty series of fresh-water pearls in (of all places) the Mining Building; and Tiffany has a fine series of pearls in their natural state, beside those worked into ornaments—but this is about all that is visible in this line, excepting the series of shells contained in the Ward exhibit in the Anthropological Building."

In the Government Building the Bureau of Animal Industry has an admirable exhibit, which shows how well science may be combined with practice. Part shows the distribution of animal life according to elevation, and part animals which are either beneficial or injurious to agriculture. Most noticeable here is the exhibit of the Entomological Bureau. "Probably no portion of the whole Exposition better illustrates the scientific modern method of presenting such matters to the public eye. The insect is shown fully labelled in all its stages and varieties, with its food plant and cocoons, with illustrations of its ravages when injurious, or beneficial methods of work when useful, supplemented by a small map showing its geographical range, and often, when of economic importance, accompanied by enlarged anatomical models. Nothing more clear, instructive, and satisfactory can be imagined. The experiment stations of the several States in the Agricultural Building show a collective exhibit which is very creditable, though less efficiently displayed than that made by the Government; and there are the usual miscellaneous collections of attractive butterflies, etc., to be found in many of the State exhibits and in those shown by foreign Governments."

The exhibits and collections hitherto alluded to fall, for the most part, under the head of semi-scientific. Those of deeper scientific

interest are chiefly to be found among the Educational Exhibits in the Manufactures and Liberal Arts Building. Here such institutions as Harvard, Johns Hopkins, Princeton, Columbia College, and the University of Pennsylvania illustrate the methods of their science-teaching and of the original research carried on under their auspices. To a scientific man this is the most interesting part of the whole Exhibition; for we are all of us students, and a few of us are teachers. Still one must regret that so small a part of the Exhibition is on these lines, and that foreign countries are so little represented. Japan, indeed, gives evidence of her new birth in this direction as well as in others; but where are the Laboratories and Museums of our old Universities? where is the Royal College of Science? where are Bonn, Heidelberg, Freiburg, Liége, Paris, Upsala, Dorpat, and Bologna? Surely the heirs of Galileo need not shun the land of Columbus. What an exhibition rises before the scientific imagination at the mere mention of these few names! But, alas! men of science are too poor even to advertise themselves, so we must make the best of Chicago, and that which the American Universities have been good enough to do for us.

From these exhibits each visitor will carry away thoughts in accordance with his own needs. The following are what appear in my note-book. A giant microtome, used chiefly for cutting microscope sections through the entire brain, exhibited by the University of Pennsylvania. The object to be transected is fixed at the end of a very heavy lever and is allowed to sink down upon the edge of a broad knife, the blade of which is parallel to the side of the lever. The section as it comes off is caught on a sloping sheet of paper. The Biological Laboratory of this University makes a regular practice of selling photographs of preparations, specimens, and drawings; and a catalogue of such, which might prove useful to lecturers, may be obtained from the Secretary. Lecturers will also be interested in some models of brains exhibited by the Physiological Department of the Loomis Laboratory of the University of New York City. These models are first of all constructed in clay; the clay is then covered over with strips of newspaper soaked in glue, which form a shell about a quarter of an inch thick. When the structure is quite dry, the clay is removed, and the hollow paper model remaining is appropriately coloured in oils. Such models may be made any size, are very light, and will stand rough usage. In the Harvard Zoological Department, W. McM. Woodworth exhibits some enlarged wax models of microscopic objects which are constructed from thin sections by an ingenious method. A camera drawing is made of each section in the series on some thin paper. The drawing is then attached to a sheet of wax which bears the same proportion to the thickness of the original section as the drawing does to its area. Each sheet of wax is trimmed to the outline of the drawing, and the successive sheets are then stuck together. The details of manipulation, which cannot be described here, are fully illustrated by specimens and apparatus.

There is no regular exhibit of scientific instruments, except that shown by several German firms in the Electrical Building. Here, too, are the excellent anatomical and embryological models made by Fr. Ziegler, of Freiburg i/B. His series now numbers 279, and costs about £120: which is not dear, considering its educational value.

Under the auspices of Harvard there are carried on in the Anthropological Building an Anthropometric and a Psychometric Laboratory. Here, in return for a dollar, one may have almost every physical and mental character tested. With the anthropometric tests Mr. Francis Galton has pleasantly familiarised us in England; but the psychometric ordeal is somewhat of a new departure, worthy of a more detailed account than is possible in this article. The casual but curious visitor to Chicago is constantly checked by some psychologic puzzle, either a pathetic appeal to choose his favourite colour among several crude abominations, or an entreaty to determine which of several meaningless geometric figures most appeals to his artistic sense of beauty. In a conscientious endeavour to advance Science by submitting to these mental tortures, I lost several hours that might have been as usefully employed in the Midway Plaisance.

To turn to Geology. One cannot complain of lack of material; collections of Fossils and Minerals are to be found in at least eighteen buildings. The most interesting of these are the Stratigraphical series of Fossils exhibited by the United States Survey in the Government Building; the educational collection of leading Fossils exhibited by Professor Ward in the Anthropological Building, and the very instructive set of specimens illustrating Dynamical Geology, in the Mining Building; his systematic collection of Minerals and set of Meteorites in the same building are also fine; the magnificent set of local fossils, including the Worthen Collection of types, in the Illinois State Building; the G. F. Kunz collection of Minerals and gems. A smaller collection, but one of the best shown by a private individual, consists of the Fossils exhibited by Mrs. A. D. Davidson in the Woman's Building.

The Mining exhibits of most of the States and Countries are either advertisements, commercial, or clap-trap. When the best that Great Britain can do is to hew a statue of Liberty in a lump of rock-salt, one begins to wonder whether we have ever boasted a School of Mines or a Museum of Practical Geology. True that there is a good collection of Economic minerals by Mr. B. H. Brough, which we are glad to see has been awarded a prize. I am told, too, that our Geological Survey has an exhibit somewhere; but what geologist would ever look for a geological map under the head of Liberal Arts? The German and Japanese exhibits are the only ones of any scientific pretensions. The merit of the German miners is, however, sufficiently well-known to Englishmen, and the geological work of Japan is important enough to demand treatment in a separate article.

The Standard Oil Company has an interesting exhibit in the form of a large scale model of a geological section across Ohio, Pennsylvania, and New York, showing the positions of their chief wells, and the relations of the oil-bearing shales. Except for a few geological maps, this is the only exhibit of the kind in the whole of the Fair. *Apropos* of the Oil Company, it may be mentioned that the twenty huge boilers that supply all the power on the grounds are heated by petroleum brought a distance of eighteen miles in a pipe.

In the Anthropological Building, Professor Wright illustrates by numerous specimens and photographs the relations of man to the Glacial Period in Ohio, of which he gave an account in the *Popular Science Monthly* for May of this year. Here, too, are a couple of marvellous exhibits. First, some footprints of various animals from a quarry of what appears to be Tertiary Sandstone at Carson, Nevada, among which certain large depressions, about the size of an elephant's footprint, are confidently assigned to Man. This, of course, upsets all the conclusions of the geologists and biologists. There have never been wanting cranks in America to uphold the existence of Man in any geological period, from the Silurian downwards; but we hardly expected to see their "notions" seriously admitted into the World's Columbian Exposition. And yet, close by, is an even more childish case, entitled Freaks of Nature, and containing such rarities as Noah's Canary, Mother Eve's Mitten, and Little Ham's Pegtop. At the end of the same gallery is an enormous map, illustrating the psychic lines of force that govern the structure of continents, from which the future course of events on our planet may be confidently predicted. Truly the Americans are a great nation, and the Anthropological Building is a great building, so they can afford to amuse us with a little folly.

BOTANY, considered as a Science, is not largely represented at the Fair, although the Botanist should find plenty to interest him in the Horticultural, Forestry, and Agricultural Buildings. In the first of these, the Japanese garden, with its dwarf trees, attracts some attention. Here one may see a pine tree 100 years old, and only 2 ft. high. The process of dwarfing appears to consist chiefly in eliminating a large number of the leaves and pruning the fresh shoots. The Forestry Building is chiefly devoted to polished woods, many of which are also to be found in the various State Buildings. The most interesting botanical exhibit is probably the selection from a wonderful series of glass models of flowering plants, shown by Harvard University. These are made by Blatschka, of Dresden, in his inimitable manner, and represent the plant as fresh as a daisy, as well as enlarged anatomical details. Nothing so beautiful exists in any other Museum of Natural History. After these, one cares little for the ordinary herbarium; but those who wish to see dried plants will find a few in the State Buildings of California and Michigan, as well as in the Woman's Building.

The scientific exhibit in the Woman's Building is, indeed, chiefly confined to botanical collections of the ordinary type, the only other specimens of interest being Mrs. Davidson's fossils. If Woman chose to divorce herself from Man, and to have a house to herself, she need not have shown herself as weaker than she is. One can imagine something a great deal better than this rather amateurish natural history display that poses as a scientific exhibit. Some papers by Miss Cora Clarke do not represent all the scientific activity of *la belle Americaine*; while foreign ladies seem to have been too shy to put in an appearance at all. To speak only of the English ones, surely the Misses Barton, Buchanan, Crane, Donald, Johnson, Raisin, and others might have exhibited something, if not for the sake of their sex, at least to the honour of their country. As for so eminent a writer as Madame Pavlov, as she has not yet described any fossil pigs, it is not to be expected that Chicago has ever heard of her.

To include ANTHROPOLOGY and ETHNOLOGY under the head of Natural Science, and adequately to describe their manifestations at Chicago, would double the length of this article. For, in one sense, the whole Fair is an Ethnological collection. There is, however, so little real science about any of the exhibits, that we need only allude to the more important. North American Indians are well represented by both recent and prehistoric relics in the Anthropological Building, where also are to be found some rare Mexican and Aztec curiosities. Reproductions of burial grounds, with real live mummies, serve as a Chamber of Horrors and attract a goodly crowd. These have been arranged by the Bureau of Ethnology, under the skilled direction of Professor Putnam, who also shows a fine series of human skulls. Outside the Anthropological Building is a hill, made of tinned iron, which contains an accurate copy of the caves and remains of Colorado Cliff-dwellers. The Australian and African weapons and utensils, and the Japanese prehistoric relics, are exceedingly interesting. Some of the State Buildings and a few Foreign Buildings also contain good collections of prehistoric relics; but, after all, the chief Ethnological interest of the Fair is living, and all around one, notably in the Midway Plaisance. Turks and Egyptians, Dahomeyans and Samoans, Javanese and Japanese, Chinese and Hindoos, meet and mingle in good fellowship with French and Russian, German and English, Italian and Swede, and with that perplexing product of Western civilisation—the American negro. It is all very well for the Dryasdusts to scoff at the pleasures of the Plaisance; but it is as superior to the legitimate collections of the Exposition as a zoological garden is to a row of stuffed animals, or an aquarium to a collection of fish-bones. Nor must we forget the forty beauties, in their national costumes. If the World's Fair offered no other object of interest to the naturalist, it would at least enable him to study the fair of the world.

F. A. BATHER.

III.

The Place of the Lake-Dwellings at Glastonbury in British Archæology.[1]

THE discoveries made by Mr. Bulleid in the Lake-dwellings at Glastonbury are of great historical value, and give a remarkable insight into the condition of the inhabitants of Somerset in the Iron Age. Moreover, their archæological horizon is clearly defined, and they lie close to the frontier which divides the Prehistoric Archæology from the history of the British Isles.

The dwellers in the Glastonbury marshes were spinners and weavers, and used whorls of stone and earthenware in twisting their thread, and weights to keep the warp tight on the loom while they worked in the weft with bone shuttles. The weft was pushed home with the weaving combs, which are both abundant and perfect. The weaving comb probably was the ancestor of the comb worn for ornament in the head-dress of later times. Numerous wooden fragments of a kind of frame probably represent the loom. Flax, in all probability, was the material which was woven, although no direct proof has been met with in this settlement. They used bone-needles for sewing.

They worked wood with great skill with the saw, the bill-hook, the knife, and the gouge, and probably also with the axe and the adze, although the two latter can only be inferred from the workmanship of the boards, and squared parts of the platform.

They also used the lathe, and are proved by the "chucks" of Kimmeridge Clay to have turned ornaments of Kimmeridge shale. Some of these have been discovered.

The lathe-turned vessels, some bearing the marks of a punch found in the settlement, prove that pottery-making was also carried on. Crucibles, and the remains of tuyères, imply that smelting was also carried on, and a piece of blue glass slag may perhaps imply that glass-working was also practised. A file implies also metal-working.

They used rings of jet, amber, and glass, and of bronze, and bracelets of bronze and Kimmeridge shale, and beads of glass, and fastened their clothes together with safety-pins and split-ring brooches of bronze, and with bone links similar to those found in Romano-British caves, such as the Victoria Cave in Yorkshire. They also used amulets of bone, among which is a roundel fashioned out of a human occipital.

[1] Abstract of Address, British Association, Nottingham, Anthropological Section.

Their huts were round and, as at Mount Caburn, near Lewes, composed of wattle. They grew wheat, and had sheep, cattle of the small *Bos longifrons* breed, pigs, horses, and dogs. They did not, however, rely wholly on the domestic animals for food, but, at times, ate the stags, roedeer, beavers, and otters living in the district.

They ground their corn in well-fashioned querns, and boiled the food by putting hot stones into the pots filled with cold water. It is strange to note among these the "potato stones" of the neighbourhood.

A spur of a fighting-cock renders it probable that they were given to cock-fighting like the ancient Gauls.

They rode or drove horses with iron snaffle-bits, and fought at close quarters with daggers, halberts, and bill-hooks, and at a distance with slings. Vast numbers of clay pellets (= the Roman "glandes") for slings, both burned and unburned, have been met with.

The position of the settlement in the marsh implies the fact that warfare was the normal social condition, and testifies to the danger of attack from neighbouring communities.

A fragment of a human skull, long, and with a low forehead, and strong frontal sinuses, implies that some of the inhabitants belonged to the long-headed section of the Britons. It may further be remarked that a shaft of a human humerus, gnawed by some weak-jawed carnivore such as the dog, was also found in one of the huts.

It remains now to sum up the place of these remains in British Archæology. The pottery is distinctly of Southern derivation and of the Late Celtic type, which belongs to the late period of the Iron Age, before the Roman influence had fully penetrated into Britain. Although the split-ring fibula and the bone links are identical with forms of Romano-British type, the absence of Roman pottery and of coins implies that the Roman civilisation had not yet arrived in the Isle of Glastonbury. Roman pottery, it may be noted, abounds in other sites in the district. On a comparison with the Late Celtic remains found by General Pitt-Rivers at Mount Caburn, near Lewes, it will be found that the iron tools and weapons, the earthenware "glandes," the pottery, and the various other articles, and the wattle-work, are practically the same, and belong, therefore, to the same age. The whole group of domestic animals, including the fighting-cock, is also the same in both. The safety-pin brooches, too, are of late Celtic type, and similar to that found in the Late Celtic cemetery at Aylesford, explored by Mr. Arthur Evans. We may, therefore, fix, with tolerable certainty, the age of these Lake-dwellers as being just before the time that the Roman influence was directly felt in the West of England, and certainly before the Roman Conquest. The discovery is most important. When fully worked out it will probably throw a flood of light on the history of pre-Roman Britain.

W. BOYD DAWKINS.

IV.

The Air-sacs and Hollow Bones of Birds.

NOT very long ago the problems connected with this subject were settled in a very offhand way. The heated air in the sacs being lighter than the surrounding air, made the bird a balloon, and so flight was easy. For the same object, the bones were hollow and marrowless. Thus a clear and interesting solution seemed to have been found for a great problem.

This theory has withered beneath the cruel light of fact. A bird can carry only a very small amount of air in its sacs, and the difference in weight between a few cubic inches of heated or cold air is too infinitesimal to be worth considering. The sight of an eagle flying off with a lamb ought to convince anyone who cannot otherwise be convinced, that the saving of the tiniest fraction of an ounce of weight would make no difference. True, air within the bird, whether heated or not, will expand its volume, lessen its specific gravity, and so fit it better for floating, but it could not help it to rise, and this is the real difficulty. Moreover, many birds, for instance the swallow, which fly to perfection, have all their large bones solid.

We must, therefore, look for some sounder theory, and first it will be well briefly to survey the facts. The lungs of birds open out into great membranous expansions which lie within the body-cavity next to the ribs or extend far back between the kidneys and the intestines; and, in addition to this, in many cases, the membrane finds its way into the bones as the marrow dries up, sometimes even to the very extremities of the limbs, and in some instances under the skin also, into some of the feathers and between the muscles. Anyone who doubts the connection between the chambers within the bones and the lungs, may convince himself by a very simple experiment. He can take a dead bird (any bird which has some of its bones aërated), break the humerus, and, after tying up the trachea, blow down the bone through a tube, when all the air-sacs will expand as promptly and completely as if the tube were inserted into the trachea.

Before now, a wounded bird, whose windpipe has been stopped with blood, has been known to breathe through a broken and exposed bone. It is beyond a doubt, then, that the hollow bones are lined with expansions of the bronchial membrane; it extends, in fact, even to some parts of the skull.

I shall first discuss the functions of the air-sacs proper as distinguished from their extensions into the bones. It cannot be

questioned that they exist partly for purposes of respiration. The lungs themselves are small and inelastic, and the diaphragm, whether it represent that of mammals or not, certainly does not do the same work.

By the action of muscles the body-cavity is expanded, a vacuum is created in the sacs, and the air rushes into them through the lungs. But it is difficult to believe that the air-sacs fulfil no purpose beyond that of respiration. If that is the case, they are far larger than necessary.

I have measured the cubic content of the lungs of a pigeon as nearly as I could, and found it to be $\frac{1}{4}$ inch. The aggregate length of the air-sacs was about $3\frac{3}{8}$ in., the depth about 1 in., the breadth about $\frac{3}{8}$ in. This gives a cubic content of $1\frac{17}{64}$. According to this estimate, therefore, the air-sacs can hold more than five times the amount of air the lungs would hold, even if they were mere bags. And probably this estimate is decidedly below the mark. This disproportionate amount of air is of some use in breathing. When the bird exhales, comparatively fresh air from the sacs is driven into the lungs, so that exhalation and inhalation alike renew the supply of oxygen; but even when this is allowed for, the air-sacs are far more spacious than is necessary for breathing alone. We have to consider, therefore, whether they do not fulfil some useful purpose in addition to their respiratory functions, and if we reflect upon the subject, it is difficult to avoid coming to the conclusion that a bird's temperature is regulated mainly by the lungs and their extensions. This will seem more than probable if we consider the means by which the temperature of the human body, when in health, is kept almost at the same point, however much that of the surrounding air may vary. Heat is lost (1) through skin by conduction, radiation, and evaporation; (2) by respiration; (3) to a small extent through the excreta. Besides this the temperature of the body is partly controlled by the vaso-motor nerves which regulate the flow of blood to particular parts. And, that this is not the only way in which the nervous system governs the temperature, there is indirect evidence in the fact that when a warm-blooded animal is subjected to urari poisoning it behaves like a cold-blooded animal. It has no longer any power of generating heat within itself in order to withstand external cold, or, when exposed to heat, of keeping itself cooler than the surrounding atmosphere.

Whatever the exact nature of this nervous apparatus may be, it is certain that in man the skin plays a very important part in the regulation of temperature, and that it is mainly by evaporation that it does its work.[1] Radiation is constant, but is much checked by

[1] Dr. Michael Foster ("Text-book of Physiology," p. 464, 1883 edition) writes: "It has been calculated that the relative amounts of the losses by these several channels are as follows: in warming the urine and fæces about 3, or according to others 6 per cent. By respiration about 20, or, according to others about 9 only per cent., leaving 77, or alternatively 85 per cent., for conduction and radiation and evaporation from the skin."

clothing; it is only occasionally that we lose much heat by conduction, for instance, when we touch cold iron; evaporation never entirely ceases, and it varies in amount according to the needs of the body. Man, in fact, is like one of the porous earthenware pots used in India for cooling water. Put them in a hot, dry wind, and, the rate of evaporation increasing, the water cools all the more rapidly, and the Sahib's bath is ready all the sooner. Great heat can be endured if only it is dry heat. A French physiologist once stopped a considerable time in a stove heated to 160° Fahr.; but this is far below the record. In 1760, according to the testimony of two French academicians, a woman entered a stove heated to 237° Fahr. and stayed there 12 minutes. Doctors Fordyce and Blayden were able to remain in a chamber heated to 260° Fahr. I have been told that a man who earned his living by feats of this kind found himself compelled to rush precipitately from the heated oven because some one, who was more scientific than kind, had placed a can of hot water in one corner. Everyone knows how oppressive the heat of a hot-house is. The heat of the vapour-baths in Russia is said sometimes to rise to 116° Fahr.; but between this and 260° there is a great gulf. When the air is moist evaporation is checked, and the human system has greater difficulty in keeping its temperature down to the normal.

Birds have, probably, nerves or nerve-fibres similar to our own exercising a control over their temperature; but evaporation from the skin cannot go on except to an inconsiderable extent since they have no sweat glands. Radiation on any large scale is prevented by the thick covering of feathers; but it is a question whether one object of the bare patches, called apteria, may not be to allow of the free access of air. Their temperature is very high, varying from 100° Fahr. to 112°. Every warm-blooded animal, we may take it for granted, has an efficient heat-regulating system. The higher its temperature the more efficient, we may assume, the system to be. What, then, in birds takes the place of radiation from the skin? We may get a hint from watching a dog when taking vigorous exercise. Dogs perspire through the tongue, and also, I believe, through the feet, but not through the skin generally. The greatly-increased rapidity of breathing is the chief means by which they keep their temperature down. All the expired air is about the temperature of the body. From this the cooling effect of rapid breathing may easily be gathered; and here we have, I believe, the explanation of the great size of birds' air-sacs, and also, perhaps, of the long ramifications of the trachea which we find, for instance, in the crane. The greater the amount of air breathed in and out, the more the body will be cooled. In man and in birds the method is in reality the same; but in birds evaporation and radiation take place mainly from the lungs and air-sacs, in man mainly from the skin. If we watch a bird's breathing we obtain evidence of this; when standing still it breathes about 20 times in the minute, whereas an adult man in a sitting

posture takes only from 13–15 breaths in the same time. During flight a bird's breathing must be far more rapid, and it is impossible to avoid the conclusion that its temperature is kept equable by this means.

There remains the difficult question of the aëration of bones. It is possible that this also may be of some slight use in the regulation of temperature; but if this is so, it can only be a very secondary object, since the air cannot be expelled from them at will. Before proceeding to discuss the problems connected with pneumaticity, I will briefly set down the main facts.

(1.) Many small birds that are first-rate flyers have either marrow in all the large bones, or else in all except the humerus.

(2.) Most of the big strong-flying birds have a great deal of aëration.

(3.) The hornbills, which, according to good observers, are very poor flyers, are as pneumatic as any birds, or, perhaps, more so than any.

(4.) Birds which dive have solid bones or only the humerus aërated.

(5.) Birds which spend much of their time in the water without diving have, at least in all the cases which I have been able to investigate, nearly all the bones solid.

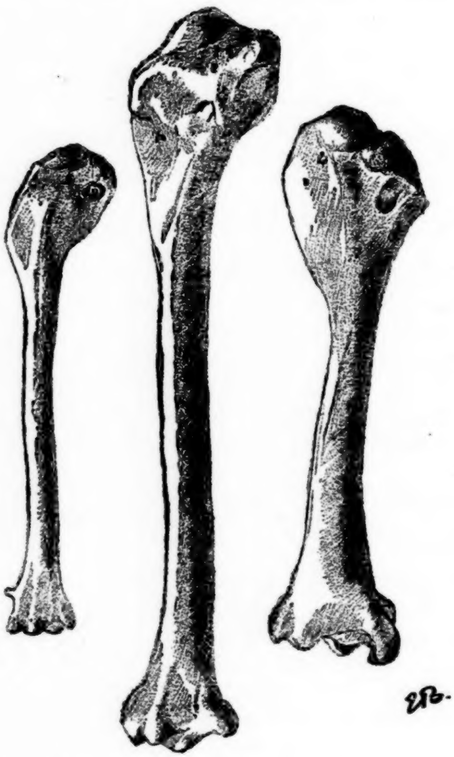
(6.) There are great differences between nearly-related species, *e.g.*, the gannet has an extraordinary amount of aëration, while its near ally, the cormorant, has only the humerus pneumatic. The hornbill is not very distantly related to the swift, which is singularly deficient in aëration.

(7.) The bones of birds that are highly pneumatic are, relatively to their length, larger in girth than those of birds in which little aëration is found.

To find one's way through these facts is not easy. But one point must strike anyone. Great pneumaticity might be an inconvenience to a diver. As it is, he can regulate the amount of his body that appears above the water, sometimes sinking till no more than his head is visible. This, no doubt, he effects by driving the air from his sacs. But aërated bones would certainly not help him to vary his specific gravity, and they would make it more difficult for him to swim under water. Probably, the marrow in the bones serves a very important physiological purpose. Divers are frequently exposed to great cold when in the water. They are protected against this by a peculiarly thick coat of feathers, and by a deep layer of fat beneath the skin; and I cannot help thinking that the marrow also helps to maintain their warmth. It is held that in it a large proportion of the red blood corpuscles are generated, and unless they are very thick in the blood, a high temperature cannot be maintained. But if the marrow is a factory of red corpuscles, what substitute for this have birds whose bones are marrowless? Though, as a rule, exposed to less cold than water-birds, they have a

very great power of generating heat. Birds, as a class, have more red corpuscles than any other animals. Is the spleen, the other great red corpuscle factory, more developed in birds which have little or no marrow? These physiological problems are of the greatest interest, and I only wish that I were in possession of facts that would throw light upon them.

Putting physiology out of sight, I am going now to consider why it is that, among birds of powerful flight, we find differences so great



a. b. c.

Humerus of Pomatorhine Skua (*a*), Vociferous Sea Eagle (*b*), and Rhinoceros Hornbill (*c*).

in the amount of aëration, and why such a poor flyer as the hornbill, is, in respect of bones, so well equipped for aërial navigation. To put physiology aside, is to assume that if hollow bones are advantageous to a bird, Natural Selection can bring it about that they become hollow, and that the bird is able to dispense with the marrow. This would indeed be a bold assumption, did we not know that it is an accomplished fact. The bones are hollow and the processes of life continue.

We shall find that as the wing lengthened, so as to make a longer and stronger stroke possible, the bones became larger in girth, larger not only absolutely, but in proportion to their length; and that a decrease of weight accompanied the increase of strength, mainly through the drying up of the marrow, but partly through a reduction, if we allow for the increased size of the bones, in the thickness of the hard, osseous shell. I shall give, first, a few measurements to show that in the case of birds whose bones have little or no aëration, the girth of the bones is, relatively to the bulk and weight of the body, considerably less—

		Girth of Humerus. inch.			Girth of Humerus. inch.
Bones highly pneumatic.	Screamer	$1\frac{5}{8}$	Bones very little or not at all aërated.	Logger-headed Duck	$1\frac{1}{16}$
	Rhinoceros Hornbill	$1\frac{3}{8}$		Scoter Duck.. ..	$\frac{13}{16}$
	Golden Eagle ..	$1\frac{5}{8}$		Nestor Parrot ..	$\frac{3}{4}$
	Vultur monachus ..	$2\frac{1}{4}$		Red-throated Diver	1
	Marabou Stork ..	$2\frac{15}{16}$		Spur-winged Goose..	$1\frac{5}{16}$

These measurements speak for themselves, even without any exact statement of the weight of the birds; but the following illustration and the accompanying tables will do more to explain the problem of hollow bones. The shoulder bones of a skua gull, which has scarcely any aëration, of an eagle and a hornbill, both of which are highly pneumatic, are placed side by side. The greater girth of the hollow bones in proportion to their length is at once clear. But to bring this out more clearly, I have taken the wing-bones of the skua as a model and calculated what would have been the length of the same bones and of the whole wing in the eagle and the hornbill, if they had been built on the same lines:—

	Girth of Humerus.	Girth of Ulna.
Pomatorhine Skua	$\frac{25}{32}$ inch	$\frac{5}{8}$ inch
Vociferous Sea Eagle	$1\frac{5}{16}$,, ...	1 ,,
Rhinoceros Hornbill	$1\frac{3}{8}$,, ...	$1\frac{1}{4}$,,

	Humerus.		Ulna.		Aggregate length of Wing bones.	
	Actual Length.	Length proportionate to girth.	Actual Length.	Length proportionate to girth.	Actual Length.	Length proportionate to girth of Humerus.
Skua	$4\frac{3}{8}$	—	$4\frac{9}{16}$	—	$13\frac{5}{8}$	—
Eagle	$6\frac{3}{4}$	$7\frac{7}{20}$	$7\frac{15}{16}$	$7\frac{3}{10}$	$20\frac{7}{16}$	$22\frac{1}{20}$
Hornbill	$4\frac{5}{8}$	$7\frac{7}{10}$	7	$9\frac{1}{8}$	$15\frac{7}{8}$	$23\frac{48}{50}$

Thus, if in the eagle's humerus length were proportioned to girth, as in the skua's, the bone would be more than $\frac{1}{2}$-inch longer; on the same principle the aggregate length of the wing-bones would be greater by more than $1\frac{1}{2}$-inch. The increase of length in the case of the hornbill is far more startling.

If now we take a saw and cut the humeri of the skua and eagle from end to end, we shall find that the walls of the latter are not thicker in proportion to the greater girth of the bone. The girths are in the ratio of 25 : 42; while 3 : 4 represents the ratio of the thickness of the walls, the measurements being $\frac{3}{100}$ and $\frac{4}{100}$ inch. We can now see why small birds have so little aëration. In their case there would

be no great reduction of weight, since the exterior shell of the bones forms a great part of their bulk. In the case of a larger bird, with bones many times multiplied in size, but the thickness of the walls increased very little, the removal of the marrow will be a great advantage. This will be clear if we take two cubes, a side of one of which is twice the length of a side of the other. Then the face is four times as large and the cubic content eight times that of the smaller cube. This will be true of other figures, so that if the average diameter of one bone be double that of another, and if the length also be double, its cubic content will be approximately eight times as great. And as the walls thicken very little with increased girth nearly all the enlarged interior can be filled with air. Clearly, therefore, a large bird has much more to gain by dispensing with marrow than a small one.

The eagle, then, has gained in lightness. It must also have gained in strength, for increased length of wing means an altogether disproportionate increase of work. The longer the wing, the greater the pace at which its extremity will move, and the resistance of the air increases as the square of the velocity.

The hornbills are a puzzle. The extreme shortness of the hand bones, a ridiculous anticlimax following upon so grand an ulna and so portentous a humerus, might suggest that they were once better flyers, and that the wing is slowly undergoing reduction. But the mountainous beak seems to show that colossal bones are an ancient heritage of the family, and that even feeble flight might have been difficult had they not become hollow. In either case they have been very great gainers by aëration.

Anyone who wishes to see clearly the relation in birds' bones of slimness to solidity, and of large girth to aëration, should inspect collections such as those at the Royal College of Surgeons, or at the Natural History Museum at South Kensington, where a large number, representing different families, may be seen side by side. It is easy, then, to see that big, long-winged birds have wing-bones thicker in proportion to their length in order to bear the far greater strain upon them: the aëration of the bones has obviated the natural increase of weight, which would have been a serious hindrance. But there remains the perplexing physiological problem: what organ of the body does the work that, in mammals, and presumably in birds with solid bones, is done by the marrow?

F. W. Headley.

V.

On the Ætiology and Life-History of some Vegetal Galls and their Inhabitants.

THIS paper is to be regarded as of a preliminary character, merely treating of the subject of galls in its more popular aspects. It embodies the results of observations made by the writer in his scanty leisure of the past twenty-five years.

With regard to their origin, galls (in the restricted sense in which the term is here applied) are complex organisms, resulting from the co-operation of a plant and an animal; and to determine the extent and *modus operandi* of these two factors in their production is one of the many interesting problems which this study suggests, but for the solution of which no complete answer can as yet be given. Why, for instance, from the action of one species of insect, a large, irregular excresence should be produced; or why, from that of another, a smooth, spherical gall, or a scaly bud, or a circular disc, is a mystery which, for the present at least, science is powerless fully to unravel. It is, however, but a special instance of the universal problem, as to the cause by which normal organic structures are produced in normal organisms.

Though abnormal with regard to the plant, inasmuch as their presence is exceptional and foreign to the performance of its proper functions, galls, in themselves, are nevertheless as normal as any other organisms. Each has its own characteristic form, its special habitat, and its proper office.

Composed at the outset, like all vegetable growths, of cellular tissue, galls undergo more or less modification as they pass through the several phases of their life-history. Some, at maturity, are hard and woody; others soft and succulent. Their colours are bright red, or green, russet-brown, or white, or yellow; with, oftentimes, such nice gradations and harmonious blendings of all these, as to give to them the aspect of ripe fruit or quasi-flowers. Though, as a rule, more or less globular in form, their shapes vary considerably. Some are smooth and regular, others rough and amorphous. In one, we have the form of an elongated cone; in another of a cup or goblet; a third is urn-shaped; a fourth discoid; a fifth reniform. Some, as the oak-apple, are composed largely of a mass of spongy parenchyma;

others, like the woolly gall of the oak, are surrounded by a thick coating of cottony-down; while in the case of the Bedegnar of the rose, we have a tangled mass of branching-filaments, compact yet free, and so compressible as to have obtained for this gall the names of the "Rose-Sponge" and "Robin's Cushion."

Few plants, it is said, are altogether free from these parasitic growths. Of those indigenous to Great Britain, a list of over 150 gall-bearing plants has been given. Such growths are found alike upon trees and shrubs and herbs. No organ escapes their deleterious presence. They appear on root and stem, on branch and leaf, on bud and flower, and fruit.[1] Every part, in turn, pays tribute to the invaders.

Of our native plants, the Oak (which is especially rich in varied forms of animal-life) produces the greatest number and variety of galls. From the oaks of Central Europe Dr. Mayr has described and figured ninety-eight specific galls.[2] Upon those of Nottingham and the immediate neighbourhood I and my friend, Dr. W. H. Ransom, F.R.S. (to whose initiative I owe my early interest in this subject), have noted some twenty-nine or thirty, out of a total of forty-one known British oak galls; while upon a single leaf I have counted more than 2,300 distinct gall "spangles"—those of *Neuroterus lenticularis*. Though many tumour-like galls are due to the action of parasitic fungi, Acaridæ, and other as yet imperfectly classified organisms, the great majority are produced by various orders of Insecta. Among these, the busiest gall-makers are found among the Cynipidæ (or Gall flies), and the Tenthredinidæ (or Saw-flies), two subsections of the Terebrant Hymenoptera. The galls of these insects are (so far as I know) invariably closed galls, from which the larvæ, in some cases, and in others the imago—then furnished with mandibulate jaws—eat out their way, when mature, and escape.

In the case of the Cecidomyidæ (or gall-midges) and other families of Diptera, as well as among gall-producing Aphides—all of which have suctorial mouths—the galls, on the other hand, though in most instances closed for a time, decay or dehisce so soon as the contained insect has reached a stage when its exit from the gall becomes necessary; but for this wondrous adaptation and provision, the home of the little occupant would, of necessity, become its prison-house and grave.

Limiting now our attention to a few only of the typical galls referred to, let us endeavour to trace out some of the more salient features of their life-history.

And first, of the "Oak Nut," or "Marble gall," as it is sometimes called. This is a simple, unilocular gall, due to the puncture of *Cynips lignicola* of Hartig, or *Cynips kollari* of Geraud, the largest

[1] Malpighi, quoted by Réaumur, vol. iii., p. 418.

[2] "Die Mitteleuropäischen Eichengallen," 1870.

member of our British Cynipidæ. It is said to be indigenous to France, and to resemble very closely the Aleppo gall, or galls of Commerce (*Gallæ tinctoriæ* of Olivier).[3]

It first appeared in this country, or was first noticed here, some 50 or 60 years ago, and was then limited to Devonshire and other of the South Western Counties. Gradually it extended itself inland, and is now, I believe, to be found abundantly in nearly all parts of Great Britain. It is the only one of the group to which the ink-gall belongs that occurs so far North as England or even Northern Germany.[4]

Each gall, with regard to position, occupies the place of a terminal or axillary bud, from the side or growing point of which it is developed; but I have occasionally met with instances in which one gall has grown out of or been superimposed upon another during its development. In its early condition, the gall is more or less cone-shaped, with pointed apex, and is slightly pubescent. It is then of a bright green colour, the surface spotted thickly over with crimson. As development proceeds, the crimson spots, which assume a scale-like aspect, separate wider and wider from each other; and the pointed apex, though persisting for a time, and in some instances altogether, usually disappears, leaving the gall at maturity more or less spherical in form, and lightish brown in colour.

In ordinary seasons, the young galls are first met with in July, and mature in August or September. This year, owing to the exceptionally bright weather of the spring, I found them as early as the 20th of June, and even at that date they had attained a diameter of from half to three-quarters of an inch.

The ovum of the gall-producing insect is, in this case, deposited in or near to the young bud of the oak at an early stage of its development, before any part of the cellular tissue is differentiated into leaves or other special organs; except, it may be, the few embryonic scales by which it is at times surrounded.

What is there, we may ask, in the casual presence of the little ovum; in the action of the developing larva; in the mechanical puncture of the parent cynips; or in the deposit of a tiny drop of irritating fluid by which it is said the ovipositing is accompanied; what is there, I ask, in any one, or more, or all of these, or, it may be, in the action of some other factor yet to be discovered, that impels those wonder-working changes by which the gall itself is produced, and by which its future growth and development are accompanied? What is it that, in the presence of this new agent, leads the succulent cells to resign their normal tendencies, and, instead of differentiating into

[3] The Gall-producer is *Diplolepis* or *Cynips gallæ tinctoriæ*.—"The gall of *Cynips tinctoriæ*," says Dr. G. L. Mayr, "occurs in the southern half of Europe, and near Vienna is frequently met with; that of *C. kollari*, however, is found as far as the German Ocean."—7 Entomol., 242.

[4] Zool. 4964, sec. 7, Entom., 245.

stem and leaves and blossoms, as otherwise they would do, to group themselves about this foreign organism, in radial and concentric layers, each endowed with new and peculiar properties, each destined to perform a new and special office?

Attempts have, of course, been made to answer these and cognate questions; but the explanations have, in great part, been little more than crude conjectures; and in no case, so far as I know, have they been of a character to meet, in any complete degree, the scientific requirements of the problem.

Redi [5]—who, like Von Helmont and the other vitalists, explained all organic phenomena by reference to the guidance of a distinct spiritual entity or Archæus—believed in a vegetative soul in each plant. This soul he considered presided at the formation of the egg, the insect, and the gall; and so determined their specific characters and their several relationships. Such views, at this day, we shall find it difficult to appreciate. They are, as Lucaze Duthiers has said, but the dreams "de l'esprit philosophique d'un autre temps." [6]

Réaumur, whose "Memoirs pour servir a l'histoire des Insects," (published A.D. 1738) contain so many curious and interesting details, was early led to refer the growth of galls to the suction of insects, having been himself much impressed by those produced by Aphides on the leaves of elms and limes. This suction, he believed, determined an increased flow of sap to the part affected, and, as a consequence, excess of vegetation and the production of the gall. As, however, both original puncture and subsequent suction not unfrequently take place in plants without any gall resulting, or without, in fact, any hypertrophy whatever, this can scarcely be regarded as the true explanation.

Neither, as Réaumur also supposed,[7] can the increased heat evolved by the developing egg be looked upon as a sufficient cause of the growth of the gall, inasmuch as no development, in the sense in which he used that term, takes place in the embedded ovum.

Malpighi,[8] who gave some considerable study to this subject, assumed a fermentation to be excited in the acid of the oak by the poison of the Cynips, and in this way sought to account for the production of the gall. He, however, like Redi and other of his contemporaries, was largely influenced by the prevalent spirit of the times, which saw, or thought it saw, fermentations in all things.

Lacaze Duthiers, whose "Researches on Galls," published in the *Annales des Sciences Naturelles* for 1853, give evidence of much painstaking study of this subject, adopts the hypothesis of a specific poison, a special *lignis virus*, as the main cause of the gall. It is, he says, a fact that all Hymenoptera have a poison gland connected

[5] Redi, born A.D. 1626.

[6] "Recherches pour servir a l'histoire de gallas," par M. Lacaze Duthiers. *Ann. des Sci. Nat.*, 3rd series, Botanique, vol. xix., pp. 284, published 1853.

[7] *See* "Memoirs," vol. iii., p. 478. [8] A.D. 1628–94.

with the ovipositor, from which it is easy to make them expel a drop of liquid by irritation. The presence of this fluid he considers to be the first essential fact in the order of causation; and he proceeds to illustrate its mode of action by reference to the analogy which he says exists between it and some well-established facts in animal and vegetable pathology. When the surgeon, charging his lancet with a drop of vaccine lymph, or with fluid "d'une ulceration syphilitique," introduces the point under the skin of a healthy subject, it is invariably followed, whenever morbid action is set up, by the reproduction of disease akin to that from which the virus has been taken, whatever may be the form of the incision, or the quantity of the fluid inserted. The specific quality of the morbid poison in these cases is, he says, an accepted doctrine—neither the result nor the evidence is questioned. Taking another series of facts, viz., those relating to the stinging of bees, wasps, scorpions, and other animals, he shows that the results, though in many respects akin to one another, are nevertheless specifically different. The swelling in one case is slight and temporary, in another it is large and persistent. It is the same with regard to the attendant pain. That resulting from the sting of a bee will continue for an hour or two, while, in the case of the scorpion, it may last for years.[9] No one, he says, denies the relation of cause and effect in such cases. Why, then, should we do so in that of the specific virus of the Cynips and the resulting gall? Once this first step taken, once the doctrine of a specific poison admitted, and we are in a position, he thinks, not only to explain the occurrence of galls in genèral, but to account also for their specific forms. No one, he says, has any difficulty in understanding how the normal forces of the plant vary with the poison, or how all secondary characters are due to the mode in which the poison acts in the developing vegetable tissues.

This view of Lacaze Duthiers is the one most generally entertained, and it has for its support the concurrent authority of Hofmeister,[10] and Darwin,[11] Sir James Paget,[12] Sir John Lubbock,[13] and other distinguished writers.

Whether or not, however, it can be regarded as the correct one, is, in a measure at least, doubtful. The analogy upon which it rests is by no means perfect. The presence of the ovum (not found in any of the cases referred to by Lacaze Duthiers) is, it may be, as necessary

[9] The injurious effects of the sting of the scorpion are said by Mr. Andrew Murray, F.L.S., to be much exaggerated, the pain being in some cases less than that of the sting of a wasp, and of incomparably shorter duration. *See* "Science Handbook of Economic Entomology—Aptera." South Kensington Museum, 1877.

[10] "Allgemeine Morphologie der Gewächse," p. 634 (1868).

[11] "Origin of Species," 5th ed., p. 572 (1869), and 6th ed., p. 6. "Plants and Animals under Domestication," 1st ed. (1868), vol. ii., pp. 382–4 418.

[12] "An Address on Elemental Pathology," Brit. Med. Assoc., Aug., 1880.

[13] "On the Origin and Metamorphosis of Insects," *Nature* Series, 1874, p. 10.

a factor in the production of the gall as is the deposit of a specific virus; while it must be remembered that galls, in many cases, result from the action of other animals than Terebrant Hymenoptera, as, for example, of *Chærmes*, *Cecydomiæ*, and *Acari*, where no such poison-gland as that referred to exists.

Very early in our investigations Dr. Ransom and myself arrived at the conclusion that another agent, as potent as that of this hypothetical virus, was essential to the production of at least some species of vegetable galls, such agent being the presence and action of a living larva—a conclusion communicated by me in a paper read before the Natural Science Section of our then Literary and Philosophical Society, on the 4th of April, 1877.

Subsequently to that date, the subject received exhaustive study at the hands of Dr. M. W. Beyerinck, of Utrecht, whose "Observations on the Early Development-Phases of some Cynips Galls" were published by the Royal Academy of Sciences, at Amsterdam, in the year 1882. In these, Dr. Beyerinck, as a deduction from the like series of facts we had ourselves observed, submitted the opinion that, in the cases referred to, the action of the Cynips larvæ, and not the injection by the parent Cynips of a specific virus, was the sole cause of gall-formation.[14]

Whether so or not, is in a measure, at least, doubtful. This, however, I think we may safely conclude, namely: that while, on the one hand, in those chemical and other forces which produce growth, greater activity is induced by the stimulus of the injected fluid—assuming such fluid to be actually present—so, on the other, those mechanical conditions which determine form in organic beings are furnished, to a large extent, by the contact of the included ovum, and by the activities of the embryonic larva.

As the nut-gall approaches maturity—which, under favourable circumstances, it may do within a fortnight or three weeks of its first appearance, early in July—the cellular tissue of which it is at first composed becomes differentiated into five principal layers. The innermost of these, surrounding the central chamber, and known as the alimentary layer, is composed of thin-walled cells, filled with protoplasm, minute starch granules, oil, and albumen. This layer disappears *pari passu* with the developing larva, which finds in its substance the food-elements necessary to its growth. At first the chamber-walls are in direct contact with the very young larva, whose increase is, in the first instance, due to absorption or to suction.

Next in order is the protective layer, or *Couche protectrice*, built up of hard, compact cells, with strong, thick walls. Around these are arranged, in radial series, bundles of elongated cells, forming a sort of *chevaux de frise* or abatis—the whole constituting a fortified casemate or stronghold for the protection of the occupant and her commissariat

[14] *See* "Beobachtungen über die ersten entwicklungsphasen einiger Cynipiden gallen" von Dr. M. W. Beyerinck, p. 8.

from the attacks of parasitic and inquiline enemies. How far it is effectual for this purpose will be seen in the sequel. Together, these several layers constitute less than one-half the diameter of the gall, the remainder being composed of loose cellular parenchyma, rich in tannin, over which extends a thin epidermis, formed of flattened cells, and resembling, in general aspect, that found on other parts of the plant. The colour of the gall, when young, is due to the presence of chlorophyl granules in the *Couche cellulaire*, these being readily seen through the thin transparent cortex. Later on, these epidermal cells become thickened and coriaceous, and the colour of the gall changes to light brown.

Passing now from the consideration of the nut-gall to that of the oak-apple—the *Pomme de Chene* of Réaumur—we find, in lieu of a single central cavity, a more or less numerous series of closed loculi, each containing a single occupant, the whole embedded in a spongy mass of cellular tissue, surrounded by a common cuticle or epidermis. Like the nut-gall, the oak-apple has its origin in a bud, terminal or axillary, and usurps the place of what, under normal conditions, would be a stem or branch. It is among the best known of our British galls—appearing early in the spring, in some cases even before the scales of the leaf-buds separate or the first tender green of the year is seen. In favourable seasons it is met with as early as the third week in April, already a blush of crimson on its delicate cheek. About the middle of May, or by "Royal Oak Day"—a time when, according to custom,

> "Custom which all mankind to slavery brings,
> That dull excuse for doing silly things,"

many of our king-and-constitution-loving ancestors were wont to use it in the decoration of their May-boughs—the oak-apple attains, in favoured situations, its full development. In very fine specimens it measures from two to two-and-a-half inches in diameter; though in size it varies considerably, and in form is less regular and globular than the marble nut-gall.

About the last week in June, or the first in July, the normal insect—*Cynips terminalis* of Fabricius, *Teras terminalis* of Marshall—makes its escape from the gall, and, after a brief but active existence of four or five days, devoted to the intercourse of the sexes, sickens and dies,—a result to which, so far as my experience goes, there is no exception: not a solitary insect survives.

At the time of our first investigation of this subject, the generally-accepted opinion was, that the eggs of *Teras terminalis* were laid in the summer or autumn of the year in which the gall appeared in the spring; and this at a time when the axillary and terminal buds of the summer shoots, close shut in their envelope of scales, had yet so far differentiated as to show a well-marked series of embryonic leaves. To this view, however, a difficulty presented itself. Our investigations showed that, at the time the insect died, these buds had not

attained that stage of development at which it was clearly seen they had arrived when the ovipositing must have actually taken place. Here, then, was a case in which fact did not agree with theory; our business, therefore, was, by further observation and experiment, to seek a solution of the difficulty. Accordingly, between the 16th of July and the 13th of December, 1876, we made a daily examination of buds from freshly-gathered twigs, by cutting these in section from apex to base, so as to expose the interior of the bud. No trace of eggs, however, appeared until the latter date, when the first group was met with. Subsequently, they were found in increasing numbers on each examination. In this way, between the dates referred to, I myself bisected 4,100 of these buds, many of them being examined under the microscope. Dr. Ransom, in like manner, opened 2,250, together 6,350 in all.

The sought-for result being thus arrived at, a new difficulty presented itself. If the gall-producing cynips died, as it was found to do early in July, how could it possibly lay its eggs in the December following? To this our own investigations gave no present answer; but a solution of the problem shortly came from Dr. Adler of Schleswig, whose researches on the alternation of generations of Gall Wasps (published in the "Zeitschrift für wissenschaftliche Zoologie" for 1880) established the fact that *Teras terminalis*, in issuing from the gall, proceeds not, as was supposed, to puncture the young buds of the oak, but to make its way to the roots of the tree, and there to deposit its eggs. In association with these a new and specific gall is formed, from which, in due course, issues a swarm of females (and none but females, for no male has ever yet been found) so unlike the parents which produced them, as to have been classed by entomologists, not simply as a different species, but as a distinct genus. These new husbandless females are altogether destitute of wings, no nuptial flight being called for, and have hence received the name of *Biorhiza aptera*.

Thus, between these two distinct and well-marked forms, we have, as you will see, a conspicuous case of heterogenesis, or alternation of generations—the bisexual giving rise to the agamic, the agamic, in its turn, to the bisexual.[15]

Let us now follow one of these agamic females, and see the kind of work she is called upon to do. Eating her way through the hard, subterranean gall in which she has passed her successive metamorphoses, she struggles forward through the intervening ground, creeping upwards to the now leafless branches of the oak. Here, by a dexterous use of her terebra, preparatory to ovipositing, she makes a transverse cut across the axis of a winter bud, above the circlet of

[15] In certain *Lepidoptera* (Psychidæ and Tineidæ) parthenogenesis appears to be a normal process; indeed, so far as known, the only process, for of some species the males have never been found. (Herbert Spencer's "Biology," vol. i., p. 215.) This agrees with *C. kollari* and other of the "Cynipidæ."

scales, so as completely to separate the cone-like apex with its appendages. In the space thus prepared (for the severed cone is still retained *in situ* by the enclosing scales), a variable number of eggs is deposited; sometimes, it may be, but a dozen or two; at others, as many as 200 to 270 or more. It is not altogether without wonder that one realises the possibility of a lodgment being found for so many eggs in so small a space, a space not one-fifth of an inch in transverse diameter across the bud, and less than one-fiftieth of an inch in depth.[16]

Each egg consists of a white, semi-transparent, pyriform body, with a long, silvery filament or pedicel at its smaller end. To the base of the detached cone, the distal ends of the several egg-pedicels are, as a rule, anchored, so as to leave the ova pendent, with their broad ends downwards, towards the fixed surface of the cut axis.

As winter passes away, and slumbering vegetation begins to feel the stir of new activities, a further phase in the life-history of the gall and the gall-insect commences. Fed by streams of nutritive sap which now circulate through the tree, the young, uninjured buds begin to swell, and the green axes to elongate.

With those, however, which have been severed by the cynips, another and a different result takes place. Should ova, notwithstanding the incision, fail to be deposited, or, if laid, perish during the winter, no growth, normal or abnormal, takes place from the divided axis. This remains brown, dry, and inactive. But if, on the other hand, healthy ova are present, and these, in due course, hatch out their living embryos, then, by the action of these upon the dormant tissues, new and peculiar powers of growth are manifested in the cut axis—powers which result in the production, not of a normal branch, but of an abnormal, tumour-like gall.

Cognate facts with regard to the galls of *Cecidomya verbasci*, found on the stamens of the Figwort and hoary Mullein (*Scrophularia canina* and *Verbascum pulverulentum*), have been noted by M. Leon Dufour.[17]

"From meteorological influences or other little-understood cause, it happens at times, he says, that the larva of this insect dies soon after leaving the egg. Then the parts in course of enlargement tend to recover; the fundamental excitement, which would be continued by the suction of the embryo, stops and fades; the swollen tissues (subjected again to normal physiological laws) contract and shrivel; the sap loses its morbid exuberance, resumes its normal course, and at last, though slowly, the stamens re-enter upon their generative functions, while the lobes of the corolla spread out and display themselves, and, in the end, recover their bright and glowing colour. Under other circumstances, where the death of the larva thus ensues—the

[16] In the ovaries of *Biorhiza aptera*, which gives rise to *Teras terminalis*, I have found 1,326 ova—in one instance as many as 1,570—the whole length of the abdomen being less than one-eighth of an inch, that of *Teras terminalis* only half as much.

[17] *Ann. des Sci. Nat.*, vol. l., 1846.

efforts of nature proving powerless to remedy the pathological turgescence—a veritable atrophy ensues, the stamens wither and the resiant gall languishes and dies."

The mode by which, in the case of the oak-apple, the essential changes are carried into effect and the development of the gall completed, is worthy our attentive study. I have already mentioned that, in ovipositing, the parent cynips so disposes of her eggs that, for the most part, they lie with their broad ends downwards towards the fixed surface of the cut axis. As spring advances, this surface becomes moistened by the rising sap; and, influenced by the process, the first range of pendent eggs is brought into direct and immediate relations with it.

About this time the embryo-larva hatches from the egg; that is, it perforates, by means of a dental apparatus already developed, the larger end of the shell, so as to come into contact with the moistened but still dormant tissue of the axis.

In this state of things new and important phenomena are exhibited. Under the stimulus of mechanical irritation, or, it may be, of chemical changes set up in the juices of the plant by a specific exudation from the embryonic larvæ, many of the cells about the woody axis become endowed with active powers of reproduction. Cell-multiplication thereupon ensues, and the formative tissue, insinuating itself between the anchored ova, gradually surrounds the escaping embryos, until each, embedded in the growing mass, is left sole occupant of its separate chamber.

In this process, the lowest eggs, or those with longest petioles, are, of course, the first to be overtaken, and, as a necessary consequence, their liberated larvæ occupy a place nearest the base of the growing gall. Others in turn are successively reached, until, finally, every embryo being embedded, the cellular mass fuses together, and presents to us the characteristic features of the oak-apple gall.

Here, then, we have a series of facts, both positive and negative, which point to the action of the embryo, and not to the deposit of a special virus by the parent cynips, as the direct and essential agent in the production of the gall. This agent, as will be seen, carries with it all the elements of a *vera causa*. It is, so far as my observations go, invariably present whenever an oak gall appears; and, though the exact mode of its operation is, for the present, undetermined, it may not unfairly be regarded as sufficient for the effect produced. This is not so with respect to the deposit of a specific virus. Many of the gall-producing insects and acaridæ have neither terebra nor poison gland; and yet, in presence of living ova, hypertrophy ensues, and a veritable gall appears.

Granting, for the sake of illustration, the existence and potency of such virus, ought we not, in this case, to expect that, even in the absence of egg or living larvæ, the normal energies of the fluid would be exerted, and, in the end, a gall—destitute though it might be of

proper occupants—of necessity result? In my long experience, however, no facts confirmatory of this view have been discovered; nor is it probable that, under any conditions, such barren galls exist. Are we not then justified in discarding the hypothesis of a specific virus deposited by the parent cynips, and in attributing to the activities of the living embryos, combined, of course, with the normal forces of the plant, the genesis and metamorphosis of the gall?

Dr. Ransom, in his address as President of the Section of Medicine at the recent meeting of the British Medical Association at Nottingham,[18] defined a gall as a local hyperplasia due to the reaction of living cells to irritation. There were no grounds, he said, for adhering to the view, once held, that the parent insect deposits a virus with the egg. No galls were known to be due to any single act or impulse of any kind. The character of the reaction varied mainly with the irritant, but also, in a much less degree, with the tissues irritated. The irritant might inhere in the embryo *in ovo*, or in the free-hatched larva, or in both, or perhaps in an adult gall-mite, or in a mycelium, or in other more imperfectly understood organisms, animal or vegetal, if living; and although he granted the possibility that it was sometimes mechanical or physical, yet he thought it more probably always in some organic liquid, chemical substance, not very diffusible, produced in small quantities either continuously or at short intervals during a part of the life and development of the parasite, and having a different composition in each species.

Resting, as I think we may now do, on this solution of the problem, let us pass from the further consideration of the gall, to that of the gall-producing insect and its parasites[19]; and here we are met by one of the most curious episodes of entomological science, with a condition of things which, if not proved to be true, might well be regarded as a mere day-dream of the imagination. Nothing, in the prosecution of studies such as these, is more calculated to arrest attention or excite astonishment than the strict balance which is preserved in Nature among the various orders of animated beings. This is especially so with regard to the Insecta. Countless millions of insect-forms continually make their appearance upon the stage of existence, millions equally innumerable as constantly perish—"Comme si" (as Leon Dufour has said), "Comme si, dans le but des harmonies de la Nature, une loi de destruction devait contre balancer une loi de production"—as if the end of the harmonies of creation was, that a law of destruction should counterbalance a law of production—as if Nature, prodigal of her resources, created only to destroy; bent, as one might believe, from one aspect of her dealings, upon securing the greatest

[18] See *Brit. Med. Journal*, July 30, 1892.

[19] I desire here to acknowledge my obligations to E. A. Fitch, Esq., F.L.S., Maldon, Essex (one of the joint editors of the "Entomologist"), for the trouble he has from time to time kindly taken in identifying and naming for me the various insects resulting from oak and other galls.

happiness to the greatest number; while, from another, she would seem but to have set her heart upon devising and providing means by which to torture and destroy the sentient creatures she has made.

In this way, season by season, the greater part of the larvæ of insects, multitudinous often as the plague-flies of Egypt, are decimated by other larvæ, inquiline or parasitic —

"The unbidden crew of graceless guests."—*Virgil.*

which live upon their juices, or so rob them of their food, that they die of atrophy and inanition.

To realise this more fully, let us follow some phases of the life-history of *Cynips kollari*, the maker of the marble nut-gall.

And here, at the outset of our enquiry, we are met by the fact that no male member of this species has yet been discovered, the entire genus (in its present more restricted sense)[20] is, in fact, believed by Hartig and other competent observers[21] to be parthenogenetic or agamic. Whether this is so or not remains, perhaps, an open question. The evidence in its support is wholly negative; and though hundreds of thousands of galls of *Cynips kollari* have been examined by ourselves and others with no result but that of yielding females, the male element may nevertheless exist, and sooner or later be found, as in the case of the oak-apple and other now well-established instances, in a gall, differing in character, in date of maturity, and in position, from that in which the female appears.

Following, then, as we may now do, one of these presumably agamic females, we find her in due season puncturing the young buds of the oak, and depositing in each a single egg.

This action, apparently so simple, becomes, as we have seen, the initial factor in a series of organic perturbations, which result in the production of a gall of a persistent and determinate character. Were it not for our experience to the contrary, we might well believe that a home so strongly fortified as this is, and so lavishly provisioned, would, under all conditions of attack, hold out against invaders. This, however, is far from being so. The cynips larva, though hidden away in the very centre of the gall, is yet amenable to the incursions, the attacks, of numerous cruel enemies, for whom its property and its life become the necessary conditions of existence.

Of these, two well-marked groups especially invite attention. These are known as *Inquilines* and *Parasites*—the one phytophagous in habit, living on the vegetable tissues of the gall; the other entomophagous, or insect-eating, preying on the juices of the living host, which in this case feeds but to nourish the wolf within.

Early in the development of the gall and the gall-insect, these parasitic and inquiline enemies are alert and active. Guided by a marvellous instinct, by a perfection it may be of smell, or subtlety of

[20] This genus comprises more than 50 species.

[21] *See* Beyerinck, "Beobachtungen über die ersten entwicklungsphasen einiger Cynipidengallen," p. 139 (Amsterdam, 1882).

hearing, one of these entomophagous parasites (of which a type specimen may be seen in *Callimome regius*), discovers that a larva, condemned to become the living prey of her progeny, is at a sufficient distance from the surface of the gall to be reached by means of her fine abdominal terebra or ovipositor, with which she accordingly proceeds to puncture the gall, and on the body of the contained larva to deposit a single egg.[22]

In due course this hatches out its living embryo, the direct enemy of the legitimate possessor of the gall. This, feeding on the life-blood of its host, grows into a well-fed larva. The larva, in its turn, becomes a pupa, and the pupa an imago. Then, plying its mandibulate jaws, the mature insect proceeds to eat its way through the woody substance of the gall; and, clad in livery of golden sheen, with bright and iridescent wings, enters upon a new stage of existence; and, strong in the instincts of its race, begins, as its parents did of old, its insidious career of brigandage and death.

Nor are the dangers of the legitimate possessor of the gall limited by the operations of these sanguinary parasites. No sooner have the galls begun to grow, and the normal larvæ to feed upon the nutritive stores with which they are provisioned, than another group of enemies —near relatives of the Cynipidæ—are introduced, unbidden, into their dwelling-place, consume their food-substance, and shorten their lives. These are the phytophagous inquilines, which, pauper-like, avail themselves of others' labours, and live at their expense. Taught by an innate faculty that the home of the cradled cynips is furnished with provisions exactly suited to the early requirements of her own offspring, the vegetable-feeding *Synergus* usurps the well-stored sanctuary, and introduces there, not a single egg, as in the case of the *Callimome*, but a dozen or more, whence issues, in due course, a tribe of greedy larvæ that—to quote M. Leon Dufour—"vont realizer le '*sic vos non vobis*' de Virgile."

Thus the normal tenant of the gall, if not fortunate enough to escape, finds itself in this most dire dilemma—either to be eaten alive by its direct parasite, the *Callimome*; or to die of starvation from the action of its enforced messmate, the *Synergus*.

Nor are the ends of Nature yet fulfilled. The Inquilines, battening on their stolen viands, suffer, like the rightful owner, from parasitic enemies, which, consuming the invaders, become, in rough and barbarous way, the avengers of the dispossessed and suffering aborigines.

But here, again, as if to punish wrong, and work retributive justice, these parasites themselves are preyed upon by other parasites; losing thus in turn their own lives, as they before had sacrificed the lives of others. This is due, in great part, to the action of a new set of spiculiferous parasites, lower in the scale of creation than the

[22] *See* M. Leon Dufour, *Ann. des Sci. Nat.*, 3rd series, Zool., vol. I.

Chalcididæ, and which include in their number some of the most minute and slightly-formed of all the Hymenoptera. These are the Proctotrupidæ, several of which, belonging to the genus *Ceraphron*, are connected with the oak-apple, where I have found them in all stages of their development. Unlike the *Callimome*, these *Ceraphrons* do not lay a single egg on the *outside* of their host, but, on the contrary, deposit a number *within* its body. Here the contained eggs are shortly hatched, and the resulting grubs begin to devour the fatty substance of the attacked larva, until, in its dry and inflated skin, they find a comfortable, cocoon-like home in which to pass their further metamorphoses.

Perhaps, in their case, as in that of their predecessors the Callimomes, other families of entomophagous parasites attack and thin the ranks of the Proctotrupidæ, and in so doing maintain, though, as it would seem, under conditions of more than Bulgarian atrocity, the necessary balance of organic life.

I only ask, in presence of such facts as these, that amiable, but, as I venture to think, mistaken "Zoophilists," when they declaim against the cruelties of scientific investigators, and seek by penal legislation to check the progress of human knowledge, should bear in mind the actual course of Nature's dealings, and believe that in this, as in other things, Man, like the Creator,

> "From seeming evil still educes good,
> And better thence again, and better still
> In infinite progression."

G. B. ROTHERA.

VI.

Desert or Steppe Conditions in Britain: a Study in Newer Tertiary Geology.

THOSE who, without taking an active part in geological research or controversy, attempt to follow the latest results of the science, must often be struck by the curious way in which old theories fade and disappear, without being directly attacked. Such a fate seems to have overtaken the Diluvial Theory, notwithstanding certain recent attempts to revive it; we would now draw attention to the crippled state of the allied theory of the former existence of a "Pluvial Period." That a period of somewhat heavier rainfall may have existed during some part of newer Tertiary times, we are in no way concerned to deny; but as a question of evidence it is noteworthy that all the facts formerly relied on can now be shown to bear a quite different interpretation, and that the new facts accumulated during recent years tend to demonstrate the former occurrence, in place of the supposed Pluvial Period, of one or more periods of excessive drought.

Geological text-books still teach that the wide sheets of gravel found in the river valleys of the southern half of England, where only sands and clays are now deposited, point to a former excessive rainfall. Some of them even still suggest that the rivers once filled the wide valleys from bluff to bluff. The writers apparently have never tried to calculate how much water would be needed to fill these sloping valleys, and are unaware that a pluvial period with a rainfall of double or treble the present amount would be quite insufficient, and that at least one hundred times the present fall would in many cases be needed. The existence of these sheets of gravel, and the constant occurrence of deeply excavated valleys in regions where no streams now exist, constituted the evidence on which was founded the well-known Pluvial Theory.

Let us, however, try to put on one side all preconceived views and examine afresh the evidence. We will take first the fossils. After comparing a number of collections from various old river-deposits, and working out each one separately, it is possible to arrive at a clearer idea of the climatic conditions that prevailed than could be obtained by any mere examination of museum specimens or of lists of species. Among the more striking results of the study is the

extreme rarity of fish remains, the comparative scarcity of truly aquatic mollusca, and the infrequent occurrence of perennial aquatic plants. The amphibious mollusca are common enough, especially such forms as can survive in the mud beneath a dried-up crust. With these amphibious forms are associated several continental species now rare or entirely extinct in Britain. We find, for instance, that one of the most abundant shells is usually the *Succinea oblonga*, now so scarce in Britain. We have also *Hydrobia marginata*, *Corbicula fluminalis*, *Unio littoralis*, and several species of *Helix*, all these forms having now disappeared from our islands. It might be thought at first sight that the assemblage pointed to a warmer climate; but a careful analysis of the list does not support this view, and the only character which the species possess in common is that all of them now live in sunnier and drier regions than ours, though not necessarily in warmer ones. The Pleistocene mammals found in Britain also point in the same direction, though not so decidedly, for many of them belong to extinct species whose former habitat is unknown to us. We notice, however, two or three species, such as the Saiga antelope and certain of the small rodents, which belong distinctly to the desert fauna of Central Asia. When the corresponding strata are traced eastward into Central Europe the evidence becomes much stronger, for Professor Nehring has discovered in Germany a mammalian fauna corresponding closely with that now inhabiting the Central Asian steppes.

If the loess of Central Europe be examined, it is found rarely to contain aquatic mollusca, but to yield in myriads such species as love sand-dunes and dust. The loess, as Baron von Richthofen has shown, is a desert-deposit such as now drifts before the winds in the dry regions of Central Asia. Thick dust-deposits like the loess do not extend so far to the west as Britain, where the climate must always have been comparatively moist owing to the proximity of the ocean; but in England there are inland deposits of blown sand, where sand does not now drift owing to the growth of vegetation, and these also probably point to drought. In the composition of our surface-soils we have also, I believe, evidence of the former wafting to and fro of fine material, which could not be obtained from the weathering of the underlying rock. The soil on our chalk Downs, for instance, is always full of small rounded grains of quartz, which cannot be derived from the underlying strata, for the upper and middle Chalk in this country do not yield anything but carbonate of lime, flint, and a little fine clay. The loess period seems to have affected Britain, though not so strongly as it did Central Europe.

The erosion of the valleys, undoubtedly exceptionally rapid during certain parts of the Pleistocene Period, and the formation of enormous sheets of valley gravel, remain, therefore, the sole evidence of the former existence of a Pluvial Period. Let us re-examine this evidence from a new point of view, and see what would be the necessary

consequences of the existence of a cold dry epoch like that indicated by the fossils. In the first place, with a clear sky the winter must have been much colder, and all pervious strata would become frozen and impervious to a considerable depth. Any rain falling before the thick frozen layer had melted could not sink in, but would immediately drain off the surface, carrying away with it a thin layer of rock shattered by the frost. Thus, the chalk Downs, which now yield perennial springs, even after a drought such as that of the past summer, would then yield no springs, for the rain could not penetrate. When the surface was frozen, the heaviest fall of rain would be entirely thrown off these steep slopes in a few hours, and the Downs would become channelled by ravines containing impersistent torrents, such as we now only associate with mountain regions where the rocks are impervious. As soon as the torrents reached flatter open country, the material brought down would be deposited in fan-shaped deltas, like that on which Chichester is built.

Implements made by man are not uncommonly to be met with beneath such gravel deposits, and these discoveries are often, but to me unaccountably, taken to prove the former existence of rivers at spots now high up the slopes of hills, or even near the highest points of nearly level plateaux. From the occurrence of stratified gravels above the implements in such situations, it is further argued that since Palæolithic times deep valleys have been cut out, and what were formerly the alluvial flats, have now become outliers of implement-bearing gravel capping isolated hills. Many writers even reason as though Palæolithic man were an amphibious animal, unable to live far from a river, and absolutely proving from the abundance of his weapons at certain spots, that a river must once have existed in the immediate neighbourhood. If such were the case, Palæolithic man must have been very different from existing savage races of the arctic and temperate regions. An overwhelming desire for an abundant supply of water is not a marked characteristic of these, and, as a rule, a small quantity for drinking purposes is all that they require. We have evidence that Palæolithic man hunted the big game that was then so plentiful; but we have no evidence in this country that he was much of a fisherman.

The conditions under which the implements are found, often scattered over deeply-buried ancient land-surfaces, and associated with hearths and other remains, which prove that on that spot was the site of an ancient settlement,[1] are to me more suggestive of sudden flood action than of ordinary rivers. Is it not possible that Palæolithic man may have lived on the existing hills and plateaux, and that the gravel beneath which his remains are buried is merely a local flood gravel deposited in any hollow over the frozen slopes, and not necessarily proving the existence of any river at such heights?

[1] See Worthington G. Smith, ref. no. 6.

The occurrence in chalk Downs, and in hills of similar porous strata, of deep valleys in which no streams now exist even after the heaviest rain, and the existence of enormous sheets of irregularly stratified or rubbly gravels in the lowlands adjoining, instead, therefore, of constituting the strongest evidence in favour of the existence of a Pluvial Period, seem more probably, like the fossils, to point to the occurrence of cold desert periods, when the rainfall, though small, could act much more energetically as a denuding agent. Under such conditions it is possible to understand the correlation of a fauna poor in truly aquatic species with deposits indicating violent floods. The absence of subterranean drainage would not only cause violent floods, even with a small rainfall, but would lead to the disappearance of the springs and consequently of all perennial streams except such as drained a large area. This, again, would tend to emphasise the desert character of the fauna.

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Clement Reid.

VII.

The Genesis of Mountain Ranges.

THE Origin of Mountain Ranges is one of the most interesting problems in Geology. It is only of late years that anything worthy of being called a *theory* of their origin has been formulated. This is not surprising, because their internal structure as well as their external form had to be first known. American Geologists have had, perhaps, the best opportunities of engaging in this enticing study, hence the interest it has evoked has been greater on the other side of the Atlantic than here.

Among those who have given form to our ideas on the subject Professor Joseph Le Conte has been one of the foremost. It is therefore an important event when, as President of the American Association for the Advancement of Science, he chooses for his theme a review of what is already known of mountain structure, and an analysis and criticism of the leading theories that have been advanced to account for the origin of mountain ranges.

Professor Le Conte commences by drawing a distinction between what he calls a *Formal Theory* and a *Causal or Physical Theory*. The first must precede the latter, and while the formal theory according to his view is well advanced, the physical is in a very "chaotic state."

The *Formal Theory* is summarised as follows:—(1) "Mountain ranges while in preparation for future birth, were marginal sea-bottoms receiving abundant sediment from an adjacent land-mass and slowly subsiding under the increasing weight. (2) They were at first formed, and continued for a time to grow, by lateral pressure crushing and folding the strata together horizontally and swelling them up vertically along a certain line of easiest yielding. (3) That this line of easiest yielding is determined by the hydrothermal softening of the earth's crust along the line of thickest sedimentation. (4) That this line, by softening, becomes also the line of greatest metamorphism, and by yielding, the line of greatest folding and greatest elevation; but (5) when the softening is very great, sometimes the harder lateral strata are jammed in under the crest, giving rise to fan-structure, in which case the most complex foldings may be near, but not at the crest. Finally (6), the mountains thus formed will be asymmetric, because the sedimentary cylinder-lenses from which they originated were asymmetric."

Several American examples illustrating these views were then given, and it was shown that eruptive phenomena, faults, mineral veins, earthquakes, and other minor phenomena associated with mountains, are well explained by them. To quote Professor Le Conte :—" Leaving out the monoclinal type, which seems to belong to a different category, all phenomena, major and minor, of structure and of occurrences, connected with mountains, are well explained by the theory of lateral pressure acting on lines of thick sediments accumulated on marginal sea-bottoms and softened by invasion of interior heat. This view is, therefore, satisfactory as far as it goes, and brings order out of the chaos of mountain phenomena. It has successfully directed geological investigation in the past, and will continue to do so in the future." [1]

This is a statement of the theory with which Professor Le Conte's name has been associated, and it is one that is distinctly intelligible. It, however, contains in itself a proof of the difficulty of drawing a marked line between the *Formal* and *Causal.* The "*hydrothermal* softening of the earth's crust along the line of thickest sedimentation" is a *physical* assumption to account for certain phenomena. It is also a further *physical* inference that "*lateral pressure* acting on lines of thick sediments, accumulated on marginal sea-bottoms, and softened by the invasion of interior heat," accounts for all mountain structure excepting the monoclinal type.

I am not either affirming or denying the truth of these statements—into which I shall examine further on, but simply wish to show that the confusion of the two modes of thought complained of by Le Conte in other investigators has not been altogether avoided by himself; this being the case, it is only reasonable to ask if such a hard and fast division be possible.

Professor Le Conte next deals with *Physical* or *Causal Theories*, which he seems to limit to an enquiry as to the cause of the lateral pressure with which he builds his mountain ranges.

First he describes what is known as the *Contraction Theory*, which refers all lateral pressure of the crust to the *interior contraction of the earth*: a theory which is commonly known as the "Shrunk Apple Theory." It is this: The nucleus of the earth, represented by the interior fruit of the apple, shrinks in bulk, while the crust remaining of the same size adjusts itself to the contracting globe by folding and wrinkling, like the skin of a shrunk apple. It is difficult to understand, if the physical theory of the hydrothermal softening of the crust can be included as part of a formal theory of mountain building, why the contraction theory providing lateral pressure should be put in another category. To my mind, the contraction theory is as formal as the theory of mountain building we are considering, especially as at first understood; for it was founded upon

[1] *Nature*, October 5, 1893, pp. 551–2.

an assumption afterwards proved incorrect in several important particulars. It is impossible to state any theory without involving cause and effect, and what is not observation in these theories is mostly physics.

After explaining the contraction theory, Professor Le Conte gives a summary of the objections to it, and a formidable list it is—too extensive, indeed, to enumerate in the space I limit myself to. Many of these difficulties have been dealt with in my "Origin of Mountain Ranges," especially the most salient of them, based on the existence of a level-of-no-strain at no great depth in the crust of the globe, above which the crust is in compression and below in tension. Le Conte evidently attaches great importance to this principle, and he may well do so, as it clearly follows that the bulk of the crust in compression is quite inadequate to account for the irregularities of the earth's surface.

Reade's Expansion Theories are next dealt with, and Professor Le Conte honours me with a generally fair formal statement of my views. He feels, however, that he does not thoroughly grasp my meaning in several important particulars, more especially in relation to the cumulative effects of recurrent expansion. It would savour of self-conceit to repeat my views here, indeed it is unnecessary, as the theories and investigations can be perused in their original extended form, by which a more just conception of their value, whatever that may eventually prove to be, can be formed.

After stating the objections to Reade's theories, which will be dealt with further on, *Dutton's Isostatic Theory* and *Reyer's Gliding Theory* are explained and dismissed—Le Conte finally, whether we may agree with him in whole, or part, or entirely dissent, concludes a very able address, written in the clear and charming style peculiar to himself, with the following sentence: "After this rapid discussion of alternative theories, in which we have found them all untenable, we return again to the contraction theory, not, indeed, with our old confidence, but with the conviction that it is even yet the best working hypothesis we have."

Before criticising the theories of this able investigator and writer, many of which are quite individual and original, I think it will be best to dwell first upon those general principles in which we are in agreement. They are these: (1) The leading principle, and one first stated in America by Professor James Hall, is that mountain ranges are built up out of immense thicknesses of sediment; (2) that there is a relation of cause and effect between sedimentation and mountain building which Le Conte attempts to explain in one way consistent with the Contraction Theory, and I in another in more direct relation to my Expansion Theory. This is a very important agreement, as there are still some, though in a minority, who are not prepared to admit either the fact or the relation. It is nearly seven years since I published my work, and the additions to our knowledge, too numerous to mention here, all point to the fact that Mountain Ranges are built

out of thick sediments. To go no further than our own island, Sir Archibald Geikie has clearly shown that the range of mountains in the Highlands of Scotland, the remains of which the Geological Survey has been investigating for many years, were built up of sediments many thousands of feet thick. It is the same, so far as is known, with all the existing great mountain ranges of the world, such as the Alps and Appalachians, the sediments of which they are composed being in many cases estimated at from 8 to 10 miles thick; (3) the accumulation of sediment, together with a consequent sinking of the sea-bottom, leads to a rise of the isogeotherms which first affect the strata of the crust on which the sediments repose, and next the sediments themselves.

These are the three leading principles in which we agree, but there are also minor points in which our ideas run parallel or partially so, but it will hardly be necessary to state them here.

But while our main principles so far run abreast, I attribute the elevation and folding of the chains in their initial stages, not to the contraction of the earth, but to the internal expansion caused by this heating of the sediments and the crust of the earth upon which they repose. These effects are intensified and continued until the building of the range is complete by other related and cumulative causes. It thus becomes a case in my view of *direct* cause and effect. Professor Le Conte apparently thinks that this expansion will have no effect at all in the way of elevation or folding, relying upon a criticism of Davison, which I shall presently deal with in the form stated in Le Conte's own words. The whole cause of both the folding and elevation is assumed to be in Le Conte's *Formal Theory* lateral pressure, and in his *Physical Theory* this lateral pressure is assumed to be due to the contraction of the earth. The hydrothermal softening of the earth's crust under the line of thickest sediments, in his view, determines the concentration of this lateral pressure in the localities in which it takes place.

Granting all the postulates involved, we here undoubtedly have a machinery which looks effective for mountain-building; but it is one thing to make a formal statement which is sufficient, and another to make its assumptions square with physical facts.

The theory was undoubtedly in better case when geologists were content to look upon the earth as in two simple conditions, namely, possessing an *unshrinkable crust* of unknown thickness—that is, of any thickness required for theoretical purposes—entirely in compression, and a *nucleus* whose function is to shrink. Such theories, even if afterwards proved to be wrong, are of value in focussing thought; they are often stepping-stones to the truth. When, however, we come to apply quantitative analysis, with the help of the physical knowledge of the day, to the Contraction Theory the problem assumes a different complexion. We find that the shrinking nucleus is but a shrinking shell, in thickness some one-twentieth part of the earth's

radius, while the underlying real nucleus is a heated globe of some 7,500 miles in diameter, which has not parted with its original heat or shrunk at all. The crust in compression is also found to be only a few miles thick, the compression being most active at the surface, gradually diminishing to nothing at the level-of-no-strain. These conditions of strain and stress in a cooling globe being pretty generally accepted by physicists of the day, Le Conte seeks to eke out the amount of contraction required by the theory in other ways. They are, however, so far, but formal assumptions having very little foundation in physical facts, and may fairly be dismissed until some better proof of their existence is forthcoming.

Let us, however, grant the lateral movement required by the formal theory, and consider how such machinery would be likely to work.

It is difficult to conceive in what way the lateral pressure of an enormous shell, 8,000 miles in diameter, could be concentrated in one or two places by any softening of strata under incipient mountain ranges, for it would involve a torsion, in varying degrees, of the whole crust, and a shearing over the whole area of the nucleus on which it rests; but this difficulty has been already dealt with by Dutton, so we will not dwell upon it. When, however, we come to consider that the application of the force is *external* to the lenticular plate of sediments to be acted upon, our difficulties increase. A softening of the crust along a certain axis might determine an elevation and folding in its immediate vicinity, but it would hardly shade off into those regular folds over a great area of country which are nearly always present where former sedimentation and upheaval has taken place. This would involve a refined gradation of softening that is not likely to occur in nature. I have also in my work dwelt on the fact that the pressure which has produced mountain ranges must have been internal and *centripetal*, and have illustrated this idea by reference to the domed form of anticlinals, varying from almost a segment of a sphere through an ellipsoid to an inverted canoe-form with their accompanying similarly-shaped but inverted synclinals.

It is, perhaps, only my individual opinion, but it seems to me improbable that the earth's crust could, by hydrothermal softening, be led into the symmetrical structures which characterise its surface configuration.

If, however, we look upon the pressure as *internal* to the folded area, the difficulty vanishes. A heating of the strata and crust would produce a state of compression acting throughout the mass from atom to atom, and greatest where the heat is greatest. This, it appears to me, is what is wanted to produce a folded mountain chain, and it will, as I have attempted to show in detail in my work and various papers, originate those great features of folds, foothills, and granitic and gneissic axes distinguishing all great mountain chains.

But, say my critics, your theory does not give lateral movement sufficient, we want much more; to which I have replied and sustained the affirmation by figures, that it gives more lateral movement than any of the other contending theories. I have also shown, and this view has been endorsed by no less an authority than Stefani,[2] that the amount of lateral movement required for mountain building is very much less than what is given by measuring the folds of mountain ranges and comparing their length with the base line of the chain. The assumption that the difference represents the amount of movement that has taken place is fallacious, because the reproduced arches never existed, and the strata have been lengthened by rolling out.

The subject of mountain building is so intricate and complex, and has so many aspects, that it would take much more space than is at my disposal to do justice to it; but before concluding these observations it will be necessary for me to reply to the objection considered by Le Conte to be fatal to my theory. He says, "But the fatal objection is that brought forward by Davison. It is this: sedimentation cannot, of course, increase the sum of heat in the earth. Therefore the increased heat of the sediments, by rise of isogeotherms, must be taken from somewhere else. Is it taken from below? Then the radius below must contract as much as the sediments expand, and therefore there will be no elevation. Is it taken from the containing sides? Then the sides must lose as much as the sediments gain, and therefore must contract and make room for the lateral expansion, and therefore there would be no folding and no elevation. I do not see any escape from this objection."

This, in my view, is a *formal* objection, and *formal* only. The criticism is underlaid by the profound misconception that the sediments *abstract* heat from the earth. So far from this being the case, *they conserve it and retain heat that would otherwise be wasted into space.* They act as a blanket or top-coat would do to the human body, as pointed out by Herschell and Babbage. The radial contraction of the earth is less under the blanketing sediments than under the other portions of the crust. Looked at in even this partial manner, there is practically no difference between a radial expansion of a part of the globe under the sediments—the only expansion apparently contemplated by Le Conte as effective—and a radial contraction of the surrounding crust of the earth. It is a question of *relative* movement and of *differential*, not absolute, heating. Increase of the sum of heat in the earth is entirely beside the mark. The answer is therefore simple and conclusive. The sediments neither abstract heat from below nor from the sides, they conserve and utilise what would otherwise be wasted. The flow of heat from below could not be quickened by the laying down of sediments, unless they were at a lower temperature than the surface of the earth's crust on which they were deposited.

[2] "Le Pieghe delle Alpi Apuane," p. 110, Firenze, 1889.

To be effective, the difference of temperature would have to be great —something quite impossible.

But it is not on radial expansion or contraction that the theory of the origin of mountain ranges bearing my name hinges. Reasoners on such complex problems should have the conditions fully and accurately before them.

A well thought-out theory cannot be gaily dismissed in a few short formal sentences.

The horizontal extension of aggregate groups of sediments compared with their maximum vertical depth is enormous; it may be 200 times as great. Consider what would be the result of heating a slab from below measuring 200 feet at the sides and 1 foot thick in the thickest part. If it were bound at the sides in the way the sediments are bound on the solid crust of the earth, the effect would be to throw it into domicular folds distributed in relation to the differential heating and thickness of the slab at various points. Even if it were not so held, the differential heating—the temperature being most intense at the thickest parts—would still throw it into folds through the intense stresses and strains set up. This is what happens in my view by the heating of the sediments which afterwards become mountain ranges. It initiates the movement, liberating other forces which continue the building of the chain. I cannot pretend to go into the details here, as they are of considerable complexity, and must refer those who wish to pursue the subject to my work and the several papers I have at various times during the last seven years given to the scientific world.

I must, however, before concluding, say a word on cumulative recurrent expansion, as Professor Le Conte here finds my theory obscure. This seems the more strange to me, as I actually commenced my work by giving concrete examples of the effect of recurrent expansion due to alternate heating and cooling. The folds in this way induced in sheets of lead are given as good examples and illustrated by photographs. Anyone who cares to look for such folds can see them in any lead gutter, lead-lined bath, or lead-lined sink exposed to varying temperatures. It is one of the plumber's arts to provide as far as he can for this well-known effect of frequent minute expansions; but I have shown that such effects are not confined to so ductile a material as lead. Terra-cotta copings are frequently affected in a somewhat similar manner. They go on expanding for years by infinitesimal degrees, mostly in the heat of summer, until I have seen what is technically called a "ramp"—a portion of the coping curving down to a lower level—lifted from one to two inches from its bed. Not only so, but in some cases a time comes when the bending at this point becomes a fault at one of the joints (normal fault), the expansion still proceeding. I have had a very interesting letter and photograph from Mr. John D. Paul, F.G.S.,

Leicester, recording an independent observation of his own on a coping at Leicester, showing a similar result, which he attributed to alternate heating and cooling by ordinary changes of temperature. The reason why this sort of expansion shows so much in terra-cotta copings, is because the joints are made in cement, and the whole becomes a long bar, alternately exposed to the vertical rays of the sun and to the cold.

Finally, Professor Le Conte seems to think that if expansion produces mountain elevation, contraction should bring the strata back again to the same level. This is a very common criticism with my objectors, and is due to a misconception born of looking at things in the gross—and, if I may venture to say it—in too formal a manner. The reason why the strata on contraction do not go back to their original form and level I have explained over and over again. It is this, no force on earth can pull strata back that have once been folded. The material by every expansion, however small, is pushed forward, and accumulates mostly in folds, so that the mountain range is actually piled up, by transfer of material from one locality to another, just as much as if it had been done by means of navvies and wheelbarrows.

To give another illustration, landslips are of frequent occurrence, and have been seen, or their effects seen, by many. One of those effects is to push up a fold, or pile up any loose materials, such as a shore deposit, before it. One might as well expect this material to go back again, and spread itself out over its old bed if the landslip were removed, as to expect mountain strata to do it, whether piled up by horizontal or cubical expansion, or pushed up laterally in any other way.

If thinkers would not concentrate their minds upon *vertical* expansion to the exclusion of *horizontal and cubical*, they would, I think, find no difficulty in understanding one of the leading ideas of my theory. In consequence of the extended areas of the sediments compared to their vertical thicknesses, horizontal expansion is infinitely greater in its effect than vertical.

When, however, all is said, there is no gainsaying the fact that the Origin of Mountain Ranges is a subject involving intricate reasoning, and cannot be disposed of by formal statements, however clear; for their clearness frequently arises from the exclusion of conditions which ought to be considered.

My object in making these explanations and criticisms is entirely to further the cause of a science which I have deeply at heart, and which, for its progress, is dependent on the labour of many. No one can rejoice more than I do at the valuable contribution towards the elucidation of so difficult a subject as the Genesis of Mountain Ranges with which the veteran Le Conte has favoured us.

T. Mellard Reade.

VIII.

Indexes to Botanical and Zoological Nomenclature.

THE compilation of indexes by means of which a student can find an accurate reference to the first description of a generic or specific name, entails a devotion to the subject known to but very few. To deliberately determine to spend some fifteen or twenty years of a lifetime in order that other students of science may be spared the time occupied in hunting up descriptions in more or less obscure publications, is a species of insanity that one could only wish were more common. The appearance of the first volume of Mr. Daydon Jackson's index to the genera and species of flowering plants, shows that it is possible to accomplish what appeared at first a hopelessly gigantic task, if one has the requisite energy, is provided with the funds, and is able to devote the greater part of his time to the undertaking.

In 1840–41 botanists were presented by Steudel with the "Nomenclator Botanicus," a list of plant names then supposed to be of specific value, together with their synonyms. In 1855 G. A. Pritzel published his "Iconum Botanicarum Index Locupletissimus," containing a reference to the first description of all the then known flowering plants and ferns, and he issued a second edition of this work in 1861; but botanical literature had grown to such an extraordinary extent by twenty years later that Darwin found to a worker like himself, living far from museums and libraries, that it was a most difficult task to get information as to new species and to the place of publication. He accordingly discussed the matter with Sir Joseph Hooker, who put the idea into form, and made arrangements for the manuscripts to be stored in the Herbarium at Kew. Darwin and his friend George Bentham agreed to supply the funds necessary to ensure the continuous working of the scheme, and Mr. Benjamin Daydon Jackson was chosen to carry it out. Mr. Jackson accordingly, in 1881, started the compilation, arranged for a special staff of clerks to assist him, has carried the work triumphantly through, and presented botanists with Volume I. (from A—Dend.) last month.[1] The successful termination of so grand a work

[1] "Index Kewensis plantarum phanerogamarum nomina et synonyma omnium generum et specierum a Linnaeo usque ad annum MDCCCLXXXV. complectens nomine recepto auctore patria unicuique plantae subjectis sumptibus beati Caroli Roberti Darwin ductu et consilio Josephi J. Hooker confecit B. Daydon Jackson." Fasciculus I. [Aa—Dendrobium]. 4to. Oxonii e prelo Clarendoniano. MDCCCXCIII.

must be a matter of supreme satisfaction to its compiler, and in congratulating Mr. Jackson, one congratulates also the botanical world on the immense labour saved to them in having the "Index" for reference.

While, however, giving Mr. Jackson the lion's share of the credit for this work, he himself would certainly be the last man to ignore the help of others. To the enthusiastic little band of clerks (despite the fact that they were paid for their work) a large amount of thanks are also due; while of his other helpers Sir Joseph Hooker has read the whole of the proofs and annotated and corrected the MS. as only one of his large experience could do. From the Kew authorities and from the staff of the British Museum (who have been supplied with proofs), Mr. Jackson has received invaluable assistance, and the same may be said of officials of the numerous public Herbaria, and many private collectors.

It is, perhaps, not too much to say that it is only in London that so gigantic a task could be successfully terminated in so comparatively short a time. The libraries at Kew, the collections at the British Museum, Bloomsbury (where are stored the books of Sir Joseph Banks), and the still more complete library of the Natural History Museum, present a series of botanical books equalled by no other city in the world; and these have all been fully drawn upon. Further, there are the collections of the Linnean and the Royal Societies, besides the rich stores of volumes in the hands of private individuals, all of which have been placed unreservedly at Mr. Jackson's disposal.

As a commencement of work, the generic names from Bentham and Hooker's "Genera Plantarum," were arranged in alphabetical order, and the names in Steudel's "Nomenclator," intercalated; each genus was placed in a separate wrapper, and housed in boxes with a fall-down front, so as to allow of easy reference and insertion of any new names that were indexed by the workers. These boxes of MS. ultimately reached the number of 168, and were arranged in pigeon holes.

The "Index" includes all names of flowering plants and ferns from 1735, the first edition of Linnæus's "Systema Naturæ," down to the end of 1885, and the estimated number of entries reaches 500,000.

The following are examples of Mr. Jackson's entries:—

Acnida rusocarpa, *Michx. Fl. Bor. Am.* ii. 234. t. 50.—Am. bor.

Beesha capitata, Munro, in Trans. Linn. Soc. xxvi. (1868) 145.—Madag.

Cyperus leptostachys, Vahl, ex Kunth, Enum. Pl. ii. 93=distans.

Printing began in the autumn of 1891, and has now reached the letter K, but owing to the extreme care bestowed on the proofs, it has only been possible to issue the first part at present. The book will be completed in four parts, and it is anticipated that the whole will be issued to the public by the end of 1894. One thousand copies will be published at a price of eight guineas nett (two guineas each part). It

is sincerely to be hoped that so monumental a work will not be allowed to stand still, but that it will be kept up-to-date in manuscript, both at Kew and at the British Museum. Indeed, nowadays, it is the obvious duty of a public institution to save the valuable time of its own staff by arranging for a special department where information of this kind can be obtained for the asking.

It is, perhaps, in a general review of this kind, somewhat carping to criticise so great a result, but one is impelled to notice the very serious oversight of the omission of dates in all but those entries relating to serial publications. It would have conduced to far greater perfection had it been realised that the date of a generic or specific name is of considerable importance; so considerable is it, indeed, that it is perfectly incredible that it should have been left out. But Mr. Jackson can remedy this if he has still the energy and the means to do it, by issuing with volume 4, at a slightly extra cost, a complete list of the abbreviations used in alphabetical order, together with the date and a brief extension explanatory of them. We trust he will see his way to do this. For a criticism on the scientific aspect of the work, we need but refer to a masterly article from the pen of Mr. James Britten in the *Journal of Botany* for October; with this criticism we are quite in accord. The introduction of the word "Kewensis" strikes us as unnecessary, and we would like to have read Jackson and Hooker on the cover, instead of Hooker and Jackson, but this without the slightest disrespect to Sir Joseph Hooker.

To turn to Zoology: up to the time of Louis Agassiz, zoologists had practically no book to refer to that would assist them in discovering the literary whereabouts of a generic name. Gmelin, in his edition of Linnæus's "Systema Naturæ," 1788–1790, had published a complete index to the many thousand species described in that work, but it was reserved for Louis Agassiz to publish a "Nomenclator Zoologicus," in which he gave a full reference to every generic name he could find that had been used up to his day (33,000 entries). Nothing further was available beyond the lists published in the yearly volumes of the *Zoological Record* (vol. vii. and onwards), until 1873, when August Marschall published his "Nomenclator Zoologicus," a book containing much new matter, but, unfortunately, in many respects inaccurate. On October 9, 1879, a letter appeared in *Nature* from Samuel H. Scudder, containing a notice that the writer had arranged and collated previous lists of generic names, and asking assistance towards its completion. In 1882 subsequently appeared Scudder's "Nomenclator Zoologicus, an alphabetical list of all generic names that have been employed by naturalists for recent and fossil animals from the earliest times to the close of the year 1879." This work was divided into two parts; part 1 containing a full reference to all generic names not in Agassiz, Marschall, or the *Zoological Record*; and part 2, "Universal Index," an index giving the name, author, and date of every generic

name, so far as the compiler had collected, which had been used for an animal from the earliest times to about 1880. This book, which contained 80,000 references, though necessarily incomplete, was at once recognised as of extreme value, and one may say without exaggeration is absolutely indispensable to zoologists. Scudder has promised a supplemental list of generic names discovered to have been omitted, but this has not yet appeared.

None of these books, however, offered those advantages to the zoologist that Pritzel offered to the botanist. They dealt with generic names only, while Pritzel dealt with specific names. The example set by Mr. Jackson in botany was followed by Mr. Davies Sherborn, who, on 15 May, 1890, published a scheme in *Nature*, for an index to the genera and species of all known animals. With some slight modification of the scheme, work was commenced on July 1, 1890, and has steadily progressed ever since. Accommodation for the MS. was provided at the Natural History Museum by the Director and the Keepers of Zoology and Geology, and the MS. is available for reference in alphabetical order of genera to anyone who desires such information. The plan of compilation adopted by Mr. Sherborn differs somewhat from that of Mr. Jackson. Every species has a separate and distinct slip, and every reference is taken from the original source; a volume is systematically searched page by page, and every species extracted, so that once that volume has been indexed, the compiler never requires to see it again. The slips are all made in duplicate with transfer paper, one set being sorted up into the great alphabet of genera, and the other set tied up and put away under the author of the book.

By this method not only is one able to see the whole of one author's work collected together, but this second set of slips will remain clean and intact for the final sorting, if publication is ever reached. Synonymy has not been attempted in the strict sense of the word by Mr. Sherborn; that, in his opinion, is more the duty of a specialist, but every time a species-name has been placed in another genus a reference is given, so that when complete and arranged the searcher will be able to find the history of that species-name without trouble, through all its generic vicissitudes.

The following are examples of the entries :—

subterranea Anas, J. A. Scopoli, Annus i. 1769, p. 67.

vampyrus Pteropus (L.), C. Illiger, Prod. 1811, p. 118 [Vespertilio].

panthera Felis, J. C. D. Schreber, Säugth. iii., p. 384 (1777) and 586 (1777), pl. xcix. (1776).

Working alone, and with one small grant from the British Association, the compiler has already collected about 90,000 references, and as the books are taken as far as possible in order of date from the earlier to the later, a considerable number of the rarer and more obscure publications are already indexed and available. The references are taken from 1758, the date of the tenth edition of the

"Systema Naturæ" of Linnæus, that having been found the most convenient starting point and more in accord with the ideas of foreign as well as British naturalists. The system above described (the completion of one book at a time) is apparently an especially good one, for it permits the MS. to be always complete as far as it goes, and will enable any person, at any moment, to take up the work, should it unfortunately be found necessary.

A still more gigantic proposition has been advanced by the German Zoological Society, which proposes not only to form a catalogue of all described species, but to give a short diagnosis of each as well! The vastness of this compilation overwhelms one, but that indefatigable indexer, Professor Victor Carus thinks well of the scheme. This could not possibly be done by one man, and the notion is, that specialists in every branch of natural history might undertake some family or even genus alone. The final details of Professor Carus's scheme are unknown to us, a brief notice only having as yet appeared in a recent number of the *Zoologischer Anzeiger*.

IX.

The Wilds of South-East Africa.[1]

IT is not difficult to prophesy that this handsome volume will be a distinct and immediate success. The public have anxiously waited to hear what Mr. Selous had to say on the Mashunaland question, and they receive the information at the very nick of time. But it is to a far larger section than to the political public that this book will appeal, for the boy will welcome Mr. Selous' volume with its stirring adventures as a companion volume to his "Robinson Crusoe," while the man will read it with the pleasure derived from his knowledge that it is written by one who knows the lions, elephants, and other large game as perfectly as he knows his horse.

Mr. Selous left England in November, 1881, for the Cape, and travelling on horse by way of Kimberley reached Klerksdorp, sleeping sometimes in the open, sometimes in the houses of the Boers, according to the weather. Purchasing a waggon and a pair of oxen and taking with him several Matabili boys from Klerksdorp, the traveller started for the interior, passing through the Matabili country and reaching the junction of the Loangwa and Zambesi rivers on 22 June. Here a camp was made, and for the next six months Selous was hunting and travelling in a country where lions were always prowling about, though nowhere plentiful.

Returning to Klerksdorp to pack and despatch his collections to England, the traveller laid in a fresh stock of provisions and trading goods, and by May, 1883, had once more entered Mashunaland, and pitched his camp on the Manyami River. In July he made a start south for the Sabi River in search for the white rhinoceros (*R. simus*) and Lichtenstein's hartebeest (*Alcelaphus lichtensteini*), in the hope of adding some skeletons and skins to the collections of the British Museum. But he was unsuccessful, and in November he broke up his camp and turned to the south-west, reaching Matabililand in December, where after having a disturbance with Lo Bengula over

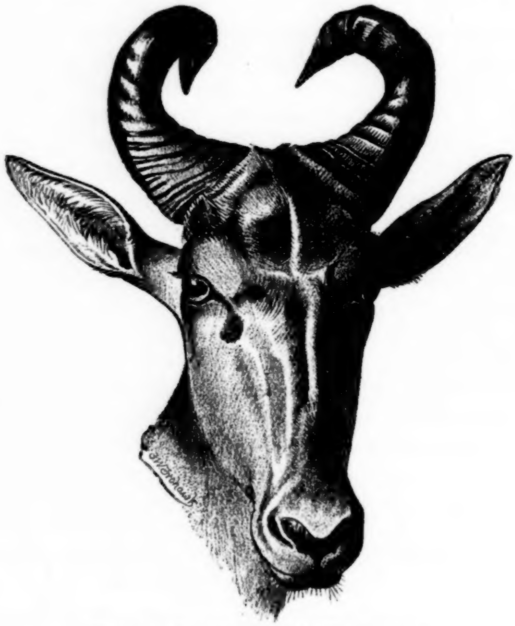
[1] TRAVEL AND ADVENTURE IN SOUTH-EAST AFRICA; being the narrative of the last eleven years spent by the author on the Zambesi and its tributaries; with an account of the colonisation of Mashunaland, and the progress of the gold industry in that country. By Frederick Courteney Selous. 8vo. Pp. xviii., 504, map, portrait, 22 plates and 35 illustrations in the text. London, 1893. Price 25s.

a hippopotamus, concerning which he was innocent, and for which he had to pay £60, he set out for the Transvaal.

Setting forth again in March, 1884, in company with W. M. Kerr, Selous decided not to hunt in Lo Bengula's country after the sorry treatment received at the king's hands, but to turn west after reaching Bulawayo, and spend his time in the northern parts of Khama's territory. Towards the end of June Mababi flat, N. of Lake Ngami, was reached, but as there was not much to be had in the way of game, the party returned to Horn's Vley, where elands, giraffes, and gemsbucks were to be found in abundance. Selous briefly describes a storm, which he saw at this place, during which the rain came down so heavily that, although the soil was a deep loose sand, the lightning showed a sheet of water, for the sand could not absorb the rain as fast as it fell. Resuming the eastward journey, the travellers followed the old waggon-spoor to Tati, and thence went back to Bulawayo. Having obtained permission from Lo Bengula, at the price of £60 (a salted-horse), for the right to hunt in Mashunaland, Selous decided to do so, especially as he had several commissions to execute for European museums in skins and skeletons of antelopes of a species more abundant in Mashunaland than elsewhere. He accordingly crossed the river Gwelo and passed through the country to Umfili and the Machabi Hills, where he fell in with a mighty herd of nearly 200 elephants, of which he and his man Laer killed six. Spending the best part of eighteen months in this district, during which time the camp was moved to the Manyami River, and excursions made in all directions, Selous was fortunate enough to secure five fine specimens of Lichtenstein's hartebeest, of which one pair are now preserved in the Natural History Museum, and a second pair are in Cape Town. In December, 1885, a return was made to Matabililand, and from thence a quick run home to England, only to return to Mashunaland early in 1887. It is interesting to read that the track which Mr. Selous' six waggons made in 1887, when passing over the ground on which Fort Salisbury now stands, was still visible in the softer soil near Fort Charter in 1890.

In April, 1888, Selous started from the Transvaal for the Zambesi, having received through Westbeech, the trader, an invitation from a missionary friend, who had once travelled with him, to visit Garanganzi country, near Lake Bangweolo. To a man of Selous' temperament, such an idea was delightful, particularly as it meant a journey through new country, and the probability of abundance of elephants, and hastily fitting out an expedition he set forth for Wankie, as the most favourable place to cross the Zambesi. Journeying north, the little party reached Minenga in the Mashukulumbwi country, and it was here that Selous met with one of his most stirring adventures—a night attack on his camp, which dispersed all his men, and left him alone in Africa with nothing save what he stood

up in, and a rifle with four cartridges. Taking the Southern Cross for his guide, Selous began his lonely journey southward, having his rifle stolen by the way, and narrowly escaping being shot. He fell in with the remains of his party near Wankie, and then found that on the fatal night twelve men had been killed and six wounded, the survivors bolting into the bush, and making south during the night, across the country. From Wankie, Selous, undeterred by the perils of the last journey, went up the Barotsi Valley as far as Lialui, and returned by canoe along the Zambesi River to Kazungula, where he arrived in October, 1888. In December, although no rain had fallen, he pushed rapidly across the desert country, and reached Bamangwato



Head of Lichtenstein's Hartebeest.

in January, 1889. After paying a flying visit to England, Selous reached Quillimani, on the Coast, in July, and conducted a prospecting party to the Upper Mazoe, travelling by canoe up the Zambesi from Mazaro to Tete. Leaving the river, the travellers struck S.W., reaching Mount Hampden (close to where Salisbury now stands) about September, returning to Tete in October. Thence journeying down the Zambesi to Vicenti, he was back in Cape Town in December.

About this time Selous threw in his fortunes with the British South African Company, and in March, 1890, we find him at Palapye and Bulawayo interviewing Lo Bengula, whose ideas on the subject of

roads through Mashunaland were being discussed with Mr. Cecil Rhodes at Kimberley, in eleven days after the interview. Returning to Palapye he formed a camp on the Macloutsie River, and examined the whole district for a strategic position, which resulted in the building of Fort Tuli in the middle of the year 1890. Towards the end of June the pioneer expedition to Mashunaland set out for Tuli, and a road was opened up through Victoria and Charter to Mount Hampden, and on September 11 the expedition, despite the hostility of Lo Bengula, was brought to an end by hoisting the British flag at Fort Salisbury. Concluding treaties with the chiefs of the southern and eastern districts occupied Selous three months, and on his return to Salisbury he plotted out a five-inch to the mile map of Mashunaland, which, completed in 1892, is now in the hands of the Royal Geographical Society. In January, 1891, we find the traveller signing a treaty of alliance and concession between Motoko, a chief of north-east Mashunaland, and the Company, and afterwards passing south to the Umtali camp, in order to cut roads from thence to the Lower Revui, and from the Odzi River to Salisbury. By the beginning of July, Selous had the entire road from Umtali to Salisbury "in good order for heavy waggons, all the bogs being corduroyed and the streams bridged." Journeying alone to Tuli to see if the waggons coming up country were in a position to bring sufficient stores for Fort Salisbury, Selous returned to Mashunaland, and was employed by the Company in laying out and making roads until May, 1892, when, there being no more work for him to do, he terminated his engagement, spent two or three months shooting and collecting specimens, and finally made his way to Beira, on the coast, and returned to England on 17 December, 1892. After a brief rest in this country, Mr. Selous returned to Mashunaland, on the news arriving of the trouble with Lo Bengula, and it is a matter of supreme satisfaction to know that so experienced and courageous a man is guarding our interests and native subjects in the wilds of Africa.

From the above somewhat lengthy account of the traveller's wanderings, the reader will have gathered that this book is one of peculiar interest, quite apart from the remarkable adventures with which it is crowded. Page after page teems with stirring exploits with lions, elephants, hyænas, and other animals, besides the risks undergone in dealing with the treacherous Matabili and other tribes. Mashunaland and Matabililand no longer convey a mere geographical expression, but are living countries, and the descriptions of their inhabitants, whether man or beast, related as they are by one who has crossed and re-crossed the country in every direction, are of surpassing interest.

The book is well got up, the illustrations are interesting, and the map of the country is of considerable value, though it suffers from the fact that the spelling of the names does not agree in all cases with

those in the narrative. It is distinctly a book to buy and put on one's bookshelves, and not one to borrow from a circulating library.

By the kindness of the publisher we are enabled to reproduce an illustration of Lichtenstein's hartebeest; and one of special interest in the book is that of a Mashukulumbwi warrior, whose singular head-dress conveys the fact that this people must necessarily live in an open country.

SOME NEW BOOKS.

An Examination of Weismannism. By George John Romanes, M.A., LL.D., F.R.S. Pp. 221. London: Longmans, Green & Co., 1893. Price 6s.

This book presents many of the characters of book-making. It has been written piece by piece—one section of it being intended for the second part of "Darwin and after Darwin," unfortunately delayed by Dr. Romanes' illness; another part consists of additions and modifications suggested by recent publications of Dr. Weismann; one appendix is a reprint of some controversy in the *Contemporary Review*, and an excellent portrait of Dr. Weismann has been put at the beginning that readers may know the features of the gentleman who is being criticised. There is in the book, therefore, the precise difficulty which Dr. Romanes complains of with regard to the writings of Professor Weismann. Inconsistencies, additions, and emendations make it very difficult to understand the exact views of Dr. Romanes. Thus in the preface the non-inheritance of acquired characters (the discussion of which is postponed to another volume) is said to be the "fundamental postulate upon which Weismann has reared his elaborate system of theories": on page 49 the fundamental postulate is stated to be the continuity and stability of germ-plasm. On page 77, while admitting that influence of a first sire upon children to a second might be reconcilable with absolute continuity of the germ-plasm by supposing the germ-plasm in the spermatozoa to penetrate an unripe ovum, he holds that an analogous phenomenon in plants goes to exclude that hypothesis and most definitely to substantiate the hypothesis that the spermatic element must exercise some influence on the somatic-tissues of the female, which in turn act upon the ovum. In the second appendix he attacks Herbert Spencer for precisely this second view, and strongly supports the first.

These, however, are mere accidental blemishes, and Dr. Romanes' general attitude can be observed. This general attitude apparently was determined by the question of acquired characters. As is well-known, when observations on the Hydrozoa led Weismann to the conception of a continuity of germ-substance from generation to generation, he raised the question as to how acquired characters could affect the germ-plasm at all. The controversy that arose turned upon the interpretation of facts, and most will agree that the controversy has contracted since it began. At first, the opponents of Weismann said that there was an overwhelming amount of evidence in favour of the general inheritance of acquired characters. Against that view Weismann and others urged that the germ-plasm was stable: unaffected by somatic changes. Now, at least, Dr. Romanes and others freely write that acquired characters are much less veritable than are congenital characters, and a large number of cases, formerly insisted upon, have been abandoned. On the other hand, Professor

Weismann has expanded those hints in previous writings, that Dr. Romanes called illogical admissions, and in his most recent book, by allowing that unequal nutrition affecting the germ-plasm may cause variation of the germ-plasm, he "admits" the action of the soma upon the germ-plasm to an extent which will probably be found to cover the residuum of facts now that they have been sifted by controversy. For this source of variation, in addition to amphimixis, and to the inherited results of the original action of the environment upon primitive organisms, brings Weismann's theory of evolution so nearly into line with what Dr. Romanes agrees to, that at first he proposed to cancel a large part of this book—but he has left it, showing how very prettily he would have fought had there been anything to fight against! But he still insists, against Weismann, that Weismann's theory requires the absolute stability of the germ-plasm. However, his examination of this question cannot be complete without the promised sections upon acquired characters, and everyone will hope that Dr. Romanes' health will allow him to publish the new volume soon.

In other matters, however, there is quite enough left for controversy.

Dr. Romanes very acutely divides Weismannism into the theory of heredity which deals chiefly with the germ-plasm, and the theory of evolution which deals chiefly with variation.

He objects to the vast elaboration of the germ-plasm with its molecules, biophors, determinants, ids, idants, and so forth, chiefly because he regards it as a work of "artistic imagination" rather than of "scientific generalisation." So far as concerns "determinants"—the particles of germ-plasm which correspond to groups of cells that vary similarly and simultaneously—he admits the great value of the conception, and he accepts the "ids" as a logical consequence of "determinants"; but he sees no reason whatever for identifying idants with chromosomes. In this, Dr. Romanes makes the same error as he did in the matter of the continuity of the germ-plasm. In both cases, Weismann started with the observed fact; the visible changes in the Hydrozoa led him to the continuity, and the actual divisions and marshallings of idants in sexual cells led him to the particulate theory of germ-plasm. And it is because his theories start from observed facts of this kind that they appeal so strongly to "laboratory naturalists."

He gives a very interesting comparison between Galton's "Stirp," and Weismann's germ-plasm, pointing out how the recent extensions (Romanes would call them "revolutions") in the latter theory bring it more into harmony with the former. Both allow that the soma arises from the germinal cells, and that in each ontogeny only a part of the germinal material is used up—the greater part being handed on. According to Galton, occasionally, but rarely, there are contributions from the soma to the stirp, and so acquired characters may be inherited occasionally: in Weismann there are never such centripetal contributions, and so acquired characters, as such, are never inherited. In the mechanism of ontogeny, Weismann insists on a peaceful disintegration of the architecture of the germ-plasm; Galton on a constant struggle between competing carriers of heredity. Weismann, in his dealing with "xenia"—the influence in plants of fertilisation, not on the fertilised cell, but on the tissues of the female, allows the possibility of the direct action of germ-plasm upon the somatic tissues, even though those tissues may belong to

another individual. Dr. Romanes calls this a surrender of the perpetual isolation of the germinal substance to a sphere of its own, and evidently holds that it goes towards establishing a reciprocal action between soma and germ-plasma. It is difficult to follow this. Weismann has of course always held that germ-plasm is isolated only so far as it does not receive impressions from the body, to be reflected upon the body of the next or after generations. The whole of his theory of the structure of germ-plasm implies that its particles have a determining influence upon the soma, and there is no difference in kind between supposing that determinants can enter and determine embryonic cells, and supposing that they may enter and determine adult cells. It is not always given to a man to know his own father, and there is no reason to suppose that a determinant can nicely distinguish between protoplasm of a developing cell and protoplasm of another cell, even although that other cell belongs to another plant.

In the theory of evolution Dr. Romanes apparently resents that the recent elaboration of Weismann's views has met many objections to them. This is chiefly in the matter of variation. Dr. Romanes writes :—" Weismann has now expressly surrendered his postulate of the absolute stability of germ-plasm! We have already seen that, even in the first volume of his *Essays*, there were some passages which gave an uncertain sound with regard to this matter. But as they seemed attributable to mere carelessness on the part of their author, after quoting a sample of them, I showed it was necessary to ignore such inconsistent utterances—necessary, that is, for the purpose of examining the theory of germ-plasm as even so much as a logically coherent system of ideas."

How charming! but with all deference to Dr. Romanes, it is, at least, as fair to suggest that Weismann, by the inclusion of "inconsistent utterances," was, in the true spirit of science, guarding himself against "logical coherence" in a shaping theory. It may be given to schoolmen, writing of such matters as the dancing of angels upon a pin-point, to be logically complete—but until, root and branch, we have eaten up the tree of knowledge, any theory of evolution or heredity that is logically coherent and complete will be scientifically incoherent and incomplete. The particular extension in question is the idea that local variations of nutrition, of which all along Weismann, as everyone else, has recognised the capacity to produce variations in *the individual*, may also produce variations in the germplasma. As Dr. Romanes rightly points out, this extension removes the great stumbling block of seeing as the cause of variation only the combination produced by amphimixis of original Lamarckian modifications of the Protozoa.

A Dictionary of Birds. By Alfred Newton, assisted by Hans Gadow, with Contributions from R. Lydekker, C. S. Roy, and R. W. Shufeldt. Part ii. 8vo. London: A. & C. Black, 1893. Price 7s. 6d. nett.

Amongst a certain section of that vast brotherhood ycelpt "Ornithologists," a work like the present has been a long-felt want; and, when it was announced that Professor Newton had in hand a "Dictionary of Birds," we are afraid that we allowed our imagination a rather lofty flight. As might be expected in such a case, when the first part actually appeared, we were bound to confess to a certain feeling of disappointment, inasmuch as we had expected a

work on a rather more pretentious scale: we felt that neither the Professor nor his colleagues were doing themselves justice. After having made ourselves tolerably familiar with the first part, however—the general scope and purpose of which, it will be remembered, was set forth in these pages a few months since—this feeling has, in many respects, given place to a sense of gratitude for small mercies, for undoubtedly it is the best book of its kind which has yet appeared, and we imagine that, not until the second edition appears will there be a better.

To attempt anything more than a very rapid survey of the part before us would be to far exceed the space at our disposal, but it shall be our endeavour to crowd as much information into that space as possible.

The first article of any importance is that on Geographical Distribution, and when we say that it covers some 52 pages, it will be seen that it is fairly exhaustive. We can bestow no further praise upon it than to say that it is in every way worthy of comparison with the excellent article on Extermination which has preceded it. Conning its pages, we feel that the whole of this most difficult subject has been so lucidly explained, that even those who take up this book for the first time will have no difficulty in mastering it.

The account of the Grebes interested us much. The figure of the Great Crested Grebe conjured up a host of old and delightful recollections of days spent, so to speak, in their company, in one of their few remaining strongholds, the Norfolk Broads. We believe that we are correct in stating that its feathers are no longer "much in request for muffs and the trimmings of ladies' dresses," inasmuch as the tide of fashion has turned in another direction.

That the gizzard of this species, and Grebes in general, is invariably filled with feathers from its breast, mixed with fish-bones, is a fact that has long been known, yet, curiously enough, it has escaped notice here. The serration along the back of the tarso-metatarsus has also passed unnoticed, though this, it is true, is a point of small importance.

The articles on Humming-Bird, Hornbill, and Kiwi are brimful of interest—especially the first-mentioned. The marvellous brilliancy of the plumage, the extraordinary variety in size and ornamentation, coupled with the Edenic life these birds are popularly supposed to lead, have tended to obscure them in a haze of poetic glamour shared, probably, by no other animal on earth. We imagine, therefore, that not a few will find it difficult to picture humming-birds "dwelling in a world of almost constant hail, sleet, and rain," or "flitting about the Fuchsias of Tierra del Fuego in a snow-storm." There is surely good reason to fear that the recording angel will soon have to inscribe the name of "*Chrysolampis mosquitus*" upon the calendar of departed species, or, in other words, it will go to swell the ranks of Birds *recently exterminated*, since "thousands of skins are annually sent to Europe to be used in the manufacture of ornaments(!), its rich ruby-and-topaz glow rendering it one of the most beautiful objects imaginable."

In speaking of the Manucode, Professor Newton says: "As with members of the Paradiseidæ generally, the nidification of the Manucodes had been shrouded in mystery, until . . . the nest and eggs of *M. comrii* were found in July, 1891 . . . ." This is correct as far as this species is concerned, but the egg of *Paradisea raggiana* was first described so long ago as 1883, by Mr. E. P. Ramsay, in the

Proc. Linn. Soc. N.S.W., vol. viii., and the egg of *P. apoda* was described from Aru in the *Zeitsch. f. ges. Ornith.* for the same year.

In the brief description of the Guinea Fowls, *Numida vulturina* has been included with those species which have "the crown bare of feathers and elevated into a bony 'helmet.'" This is, of course, an oversight, this particular species having no "helmet."

This part concludes with two extremely interesting articles on Migration and Mimicry. The former is especially fascinating, and brings together a mass of information, for which the general reader should be grateful.

Dr. Gadow's contributions can in no way be said to play a secondary part in this book. His task is by no means an enviable one, for to render concise and readable articles out of such uncompromising material as has fallen to his share to mould, is, to say the least, a difficult one; but, nevertheless, he has succeeded in presenting us with descriptions that are at once singularly clear and undoubtedly correct. We would, however, be allowed to point to the description of the humerus and the four accompanying figures as a slight exception. In the figures referred to—the left humerus of a raven—all the more important points of muscular attachment have been distinctly named, but, in the lowest figure, the crista superior is called crista "lateralis," while in the text "tuberculum superius" should read "tuberculum externum" so as to agree with the figures. So again, "tuberculum inferius" should read "tuberculum medium," for the same reason.

Now a word as to the illustrations. On every hand, the figures of Swainson have been most eulogistically praised. *Fas est dictu*—we have seen better. For instance, who would recognise the caricature on p. 401, which is supposed to represent the head of the Guinea Fowl? or the ? model for an umbrella handle on p. 406? which we are asked to believe is the head of *Scopus*. To our mind, the figures on pp. 388, and 508–10 are far-and-away more life-like and beautiful. Some of Swainson's figures undoubtedly are good, such as that of the head of the Hawfinch, but these are the exception, and not the rule. The elegant figures of the Heron, Jabiru, and Merlin are almost beyond praise, but these are not Swainson's.

Figures of bills and feet come prominently to the fore in this book; doubtless they have their value, but they are by no means so important as the prominence given them would lead the lay mind to believe. In the case of unfamiliar forms, such as the Guan, we venture to think they are of no use whatever; here, not even the entire head is given, but only the beak. The amateur must indeed have a vivid imagination if he would conjure up the remainder of the bird!

We wish it to be distinctly understood that these remarks are offered, not in any spirit of captious criticism, but, as suggestions which might be acted upon in a second edition, which, we feel sure, will be called for.

We will conclude as we began, by reiterating that this book undoubtedly fills a gap, and it is hardly likely to be supplanted by any similar work for a long time to come. W. P. P.

The Zoological Record for 1892. Edited by D. Sharp, F.R.S. 8vo. Pp. 926. London: Zoological Society, 1893.

It is no exaggeration to say that the *Zoological Record* is the most important zoological publication in existence. The absence of this

yearly volume would bring about a state of affairs too terrible to contemplate even in imagination; it is our business, therefore, to be exceedingly thankful to the editor and his staff of contributors for the great pains which they have taken in registering the immense amount of zoological work of the past year, and to the Zoological Society for bearing the expense of this necessarily costly publication. These grateful feelings on our part leave scarcely any wish to criticise; but as a matter of precedent it is a very easy task to criticise the *Zoological Record*; it has been laid down in all criticisms of past volumes which we have read, that the way to deal with this work is to search diligently for misprints; to ignore the vast amount of labour expended upon its production, and to dwell particularly upon the defects of newly-enrolled members of the staff of recorders. We, personally, happen to be of an extremely conservative disposition, and are, therefore, intense respecters of precedent; in spite, however, of natural inclination, stimulated by such misprints as "*Aelosoma*" for "*Aeolosoma*," *fluviventris* for *flaviventris*, *Naidonina* for *Naidium*, etc., we feel very strongly that the only proper way to criticise the *Zoological Record* is to point out, if it can be done, that the record is incomplete. So far as we have tested the present volume this is not the case; it appears, however, that the last volume was so to some extent, for there are a few references to papers published in 1891, which should, of course, have been included in that volume. But—particularly in this kind of work—better late than never! One of the more useful features of the *Zoological Record* is the general section with which it commences; this improvement was introduced by the late editor, and we are glad to notice that the present editor has not dispensed with a very important part of the volume.

F. E. B.

Our Household Insects. An Account of the Insect-Pests found in Dwelling-houses. By Edward A. Butler, B.A., B.Sc. 8vo. Pp. 344, 7 plates and 113 figures. London: Longmans, Green & Co., 1893. Price 6s.

We can strongly recommend this book, whose chapters originally appeared as articles in the pages of *Knowledge*. The various species of insects which have, more or less, established themselves as companions of man are described in systematic order. The structure, habits, and life-history of each insect are set forth in a manner at once accurate and popular; so that the reader who is attracted to the book will learn what material for interesting study may be found in the bodies of the humble lodgers in his dwelling. The affinities of the household insects with their out-of-door relations are indicated, and a very fair notion of the comparative morphology of the leading insect-types may be obtained by the careful student. The insects described include the wood-boring, skin-eating, and meal-worm beetles, ants, wasps, clothes-moths, cockroach, cricket, earwig, flies, gnats, flea, bug, book-lice, silver-fish, and skin-lice. The figures of the insects and their anatomy are good, and the plates are reproductions of micro-photographs prepared for lantern slides.

While the book is primarily intended as a popular work, we do not think the author will be disappointed of the hope expressed in the preface that the serious student of entomology will find it useful. The only objection is likely to be raised by the householder, who will perhaps complain that while the author describes with enthusiasm the jaws of the creatures and the depredations they commit on

furniture, food, and clothing, he has but little advice to give upon methods for their wholesale destruction.

ON HAIL. By the Hon. Rollo Russell. 8vo. Pp. xvi., 224, with 2 photographs of Hailstones. London: Edward Stanford, 1893. Price 6s. nett.

THIS book deals with "Descriptions of Hailstorms and Hailstones," "Observations of Temperature, Clouds, and Winds at great Altitudes," and "Electricity and Hail." The author then gives a digest of the various "Theories of Hail," and of "certain properties of Vapour, Water, and Ice, and conditions of the Air which may be connected with the formation of Hail," with a "Summary of characteristics of Hailstorms and Hailstones," and notes on "the Development of a Hailstorm." Appendixes on "General Weather Conditions," "Cold produced by Radiation," "Dust Particles and the form of Ice-crystals," "Types of Hailstones," etc., are also given, and the author states the conclusions arrived at from his researches.

The work can be recommended to the student, as presenting all that is known on the subject up to the present, and especially for the convenient digest of the theories of previous writers. It should also form a useful addition to the library of the general reader. It is well got up, printed in large type, and the two photographs of hailstones (actual size, one of which is 2 in. in diameter) which fell at Richmond, Yorkshire, in the storm of 8th July this year, are excellent, and show both internal and external structure.

MESSRS. CASSELL & Co. have issued part i. of a new Gazetteer of Great Britain and Ireland; it is clearly printed, it is accompanied by a neat map of the British Isles coloured to show the counties distinctly, and there is also the first section of a map of England on a scale of an inch to rather more than ten miles. The work is intended to be "A Complete Topographical Dictionary of the United Kingdom," and it professes to meet "a want hitherto unsupplied." Among the many matters on which information is promised in the preface, are notes on the Prehistoric Remains, Earthworks, &c., on the Physical Features, and (in the cases of parishes) some account is to be given of the nature of the soil.

Judging from the part now before us, we find that the articles, as a rule, are much shorter than those of Fullarton's Gazetteer of England and Wales, of which the first edition was published in 1843. It was no doubt difficult to get good accounts of the soils in each parish, consequently we find the subject treated very unequally, and not always clearly, nor accurately. The following are instances:—At Abbeystrowry, "Soil clay and artificial loam, overlying slate"; Abbotsbury, "Soil chiefly red clay" (no mention of iron ore and limestone); Aberavon, "Soil sand and gravel, overlying coal and minerals"; Abernethy, "Soil chiefly granite rock"; Aldborough, "Soil loamy, overlying clay" (no mention of shelly "crag"); Aldington, "Soil various, overlying rock"; Aldsworth, "Soil stonebrash, overlying lime and freestone." A concise and reliable account of the principal soils and economic products of the rocks in each parish would have been useful.

THE Cambridge University Press are about to publish a series of Natural Science Manuals, which will cover a wide field, some of

the books being adapted for beginners, while others will deal with special topics and will be useful only to more advanced students. The series will be divided into two sections, a Biological and a Physical. The former will be published under the general editorship of Mr. Arthur E. Shipley, M.A., Fellow and Tutor of Christ's College, Cambridge, it will include a manual of Invertebrate Palæontology by Mr. H. Woods, Demonstrator of Palæobotany at Cambridge, which is now ready; a text-book on the Practical Physiology of Plants, by Mr. Francis Darwin of Christ's College, and Mr. E. Hamilton Acton of St. John's College, which is in the press; works on Physical Anthropology, by Professor Alexander Macalister; on the Vertebrate Skeleton, by Mr. S. H. Reynolds of Trinity College; on Fossil Plants, by Mr. A. C. Seward, Lecturer in Botany in the University, and an Introduction to the study of Botany by Mr. Francis Darwin, which are in preparation. Other volumes will shortly be announced.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MR. CHARLES F. W. MCCLURE, M.A., has succeeded Dr. Osborn as Professor of Biology at Princeton College, New Jersey.

THE REV. A. IRVING, D.Sc., F.G.S., has resigned his appointment at Wellington College, Berks, and removed to Hockerill, Bishop's Stortford, Herts.

THE late Professor Newberry's name is to live in a fund which the scientific societies of New York have resolved to raise. It will be called the John Strong Newberry fund, be not less than 25,000 dollars, and the income derived from it will be devoted to the encouragement of scientific work in geology, palæontology, botany, and zoology. Professor N. L. Britton is secretary to the subscription committee.

THE Jersey Biological Station promises a quarterly publication together with an issue of microscopical preparations of the rare and less known marine organisms. The text will be descriptive of the slides issued, and will contain hints and notes on microscopical manipulation, together with original observations upon Marine Zoology. The subscription for the year, including fourteen slides, is 21s., and this issue is limited to seventy-five copies.

WE regret to learn that, owing to the transfer of political power in the Illinois legislature, Dr. Josua Lindahl has been dismissed from the post of State Geologist. Dr. Lindahl's scientific attainments are well known; he served as zoologist on the "Porcupine" expedition, and the concluding volume of the Report of the Geological Survey of Illinois showed that in him that State had secured an energetic, industrious, and capable servant.

WE much regret to learn that the post honoured by the names of Amos H. Worthen and Josua Lindahl, has been handed over to a Mr. William F. E. Gurley. This gentleman is, we understand, a collector of fossils, and his name has once appeared in so-called scientific literature as co-author of a paper with Mr. S. A. Miller, of Cincinnati, O. One more step remains for the State of Illinois—to make Mr. Miller State Palæontologist; and this step will doubtless be taken.

THE fine collection of fossils made by the late Professor A. H. Worthen, which contains very many of the specimens figured and described in the Report of the Geological Survey of Illinois, has at last been bought by that State. The specimens are at present exhibited in the Illinois building at the World's Fair. Each has a register number, and, as a very complete sale-catalogue was published,[1] all future visitors to the State Museum will easily be able to identify them.

[1] Warsaw, Ill., 1889; sold in London by Wm. Wesley & Son, price 6d.

It seems odd to us in England that a well-paid State Geologist should be able to make a private collection of 2,871 fine specimens, to treble its value by selecting 752 of these as types for the descriptions by other State officers in the State publications, and as a consequence to finally force on the State the purchase of this collection for a very large sum of money. But in America such a proceeding is not thought in the least odd; and that is the oddest thing of all.

The Indiana Academy of Science has, according to *Erythea*, determined on a biological survey of the State. The promoters intend to publish a complete bibliography of the botany, zoology, and palæontology of Indiana, to ascertain what has already been done, while the main purpose of the survey will be to make known the extent, distribution, biological relations and economic importance of the entire fauna and flora. L. M. Underwood is at the head of the botanical division, and the special work of this season in botany will be a study of distribution, particularly of the lower cryptogams and of the rarer forms of flowering plants and ferns. We trust the enthusiasm of the Indiana naturalists will endure throughout and carry to a successful issue this worthy project.

The *Transactions of the Norfolk and Norwich Naturalists' Society* for 1892–93, which has just appeared, form an excellent model for local societies. Of the 170 pages contained in this part, all except four or five relate to the natural history of Norfolk. The address of the President (Mr. H. B. Woodward) deals with the geology of the county. Then follow papers by Mr. Southwell on the Siberian Pectoral Sandpiper, Sowerby's Whale Shooting at Holkham, The Herring Fishery of 1892, and some Additions to the Norwich Museum. Mr. J. H. Gurney writes on the Immigration of the Lapland Bunting; Mr. H. B. Woodward on Caleb B. Rose, the Norfolk Geologist; Mr. Mayfield on Norfolk Slugs; Mr. A. W. Preston on Meteorology; Mr. Clement Reid on *Paradoxocarpus carinatus* from the Cromer Forest-bed; and last, Mr. James Edwards contributes an account of the Coleoptera, forming Part XII. of the Fauna and Flora of Norfolk. Sir P. Eade's note upon Tortoises is the only contribution not of local interest; and even it relates to tortoises that have lived ten years in the county, and may therefore be considered almost to belong to it.

The first number of the *Proceedings of the Malacological Society of London* has just made its appearance, edited, under the direction of a publication committee, by Mr. B. B. Woodward. In addition to the proceedings of the inaugural and subsequent meetings of the first session, it contains eight of the papers read by members. One-half of these are systematic, while of the remainder two are anatomical, so that the ground covered is fairly representative of the aims of the Society, as set forth on the second page of the wrapper. The number, which runs to 30 pp., is illustrated by two full page plates and several illustrations in the text. A footnote to page 11 makes us aware that conchologists cannot, alas! more than any other section of humanity, implicitly trust their fellow man, some smart American seemingly having robbed Mr. E. A. Smith of one of his new species, a proceeding which the publication committee "greatly regret," and reflect upon.

The fifteenth annual meeting of the Greenock Natural History Society was held on September 28, and the secretary, Mr. G. W. Niven, presented a satisfactory report. Only one paper embodying local research seems to have been read during the last session, that by Mr. M. F. Dunlop, on some rare Rotifers. The meetings are chiefly occupied with papers of a general character, and sometimes relate to physical science.

The fifty-fifth annual meeting of the Manchester Geological Society was held on October 10. Notwithstanding unusual losses by death and other causes, the membership is still well maintained, and many important communications, especially

of economic interest, have been discussed during the past session. In the early part of this year a circular was addressed to the members and the coal-owners and lessees of Lancashire and Cheshire, asking for their assistance and co-operation in the work which the Society is hoping to accomplish, of giving a fairly accurate enumeration and description of the fossils of the coal-bearing rocks of this district. Instructions were given to carefully note the exact horizon and locality from which each fossil was derived, so that an attempt might be possible to correlate the various coal seams in different parts of the county, thereby rendering valuable aid to future explorations for coal. The result of this appeal has not, as yet, been such as the Council had reason to expect, but it is hoped that the object has not been lost sight of, and that considerable additions to our knowledge of the fossil fauna and flora of the Coal-measures will yet be forthcoming from the hands of the members and others interested in the work. Several members have already responded by sending specimens, whose assistance will be acknowledged and their contributions incorporated in the next list of Fossil Plants, etc., which is being prepared by Mr. Robert Kidston, F.G.S., for publication in the Society's Transactions.

OBITUARY.

H. W. CROSSKEY, LL.D., F.G.S.

Born 1826. Died October 1, 1893.

By the death of Dr. Crosskey, Geology loses one of the most painstaking students of Glacial and post-Glacial deposits; while to the general public he was well-known as a leading Unitarian minister, and as one devoted to the educational advancement of the poorer classes. He was born at Lewes, and undertook his first pastorate at Derby. Thence he removed in 1852 to Glasgow, and in 1869 to Birmingham. While in Scotland he paid much attention to the post-Tertiary deposits of the Clyde Valley, and about the year 1855 he became associated with David Robertson, "The Naturalist of Cumbrae," and their joint labours on those fossiliferous deposits were published by the Geological Society of Glasgow. Later on they were joined by Dr. G. S. Brady in a special study of the post-Tertiary Entomostraca (or Ostracoda), and this resulted in a Monograph on the subject, which was published by the Palæontographical Society in 1874. Dr. Crosskey was the author of a series of Reports on the Erratic Blocks of this country, which were communicated during the past twenty years to the British Association.

The death is also announced of Mr. Thomas Bain, the South African Geologist, who for a long period continued the work of his father, the late Mr. Andrew Geddes Bain, in discovering so many of the fossil reptiles of the Triassic Karoo Formation now in the British Museum.

CORRESPONDENCE.

THE PRE-GLACIAL BRITISH FAUNA.

THE solution of the problem which forms the subject of Mr. Bulman's article is a very important one, both from a biological and from a geological point of view, and he has very ably argued the points in favour of a survival of a pre-Glacial Fauna and Flora in the British Islands. Since the appearance of Professor Forbes's classic essay referred to by Mr. Bulman, many additional facts regarding the existence of a Lusitanian Flora in the South of Ireland have been brought to light, and although the possibility of their post-Glacial transmission by birds is not to be excluded, it is not likely that such was their origin, since they are now known to be associated with Lusitanian animals, of which one at least (*Geomalacus maculosus*) must have reached these shores by a land passage, as both it and its eggs are almost immediately killed by immersion in sea-water. That this slug should have been accidentally carried by birds from Portugal to the South of Ireland is surely not worthy of argument. The geographical distribution of slugs, indeed, and of non-operculate molluscs, especially the subterranean species, may be most profitably utilised in the task of solving the problem of the origin of the British Fauna.

Equally important, and in many respects even more so, are the mammals, and an enquiry like that initiated by Mr. Bulman might with advantage be started, tracing the origin of the Irish species first. Familiar English species, such as the Weasel, Mole, Voles, Common Shrew, and Hare, and many others, are absent from Ireland. It has been suggested that Ireland was separated from England before these mammals had time to cross over, but among them we have some of the most quickly-spreading animals, and it is more probable that they arrived in England only long after the species at present inhabiting Ireland, which therefore formed part of another and much earlier immigration from the continent. This immigration was probably pre-Glacial, but that the second was not entirely post-Glacial seems proved by the fact that remains of some of the mammals mentioned as being absent from Ireland occur in the Forest Bed.

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ARCHÆOPTERYX.

IN reference to Dr. Hurst's interesting, but somewhat intemperately-worded article on *Archæopteryx* last month, I should like to remark that although Professor von Zittel's copy of Owen's restored figure of the hand of this bird wants one finger in the original German edition of the "Handbuch der Palæontologie," the mistake is rectified in the subsequent French edition. It is thus obvious that the author's omission was an accident, and not intentional; possibly the block was "battered" at the edge of the page.

A. S. W.

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